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## THE PHILOSOPHY OF ANIMAL COLOURS.

By Dr. Andrew Wilson, F.R.S.E.

THERE is a suggestive passage in Butler's "Hudibras," which maintains that—

> "Fools are known by looking wise,
> As men find woodcocks by their eyes."

And if the axiom be correct, that a poet is only great when he is true to nature, it must be admitted that Butler has been singularly felicitous in this metaphor. Whoever has seen a woodcock in its ordinary summer plumage may form a good idea of the truth of the poetic remark. As that bird moves about amongst the fallen leaves of autumn, the greys, and browns, and yellows of its feathers mingle so beautifully with the like tints of its surroundings, that the animal is absolutely concealed from any view but the practised eye of the sportsman. As has been remarked of the bird in question, even the very conspicuous and ornamental tail becomes hidden from view in a most singular fashion. Below, these tail-feathers exhibit a white colour tinted with a silver sheen and marked with a deep black. Nothing more conspicuous than such an ornament can well be imagined; yet the tail and its belongings are, nevertheless, wonderfully concealed. For, as the bird reposes, these under lines and tints are placed downwards; and above, the ashen-grey tints mingle perfectly with the bird's surroundings. As the woodcock, therefore, rests amid its background of wood and its foreground of fallen leaves, every line of its plumage is made to assimilate so closely with the objects around, that the bird's presence, even a short distance off, is not suspected.

The woodcock is by no means alone in this harmony betwixt its plumage and its surroundings. The sand-grouse of the deserts, for instance, exhibit a like harmony. These birds cannot be detected, even as they run, amidst the sand of their haunts—so closely imitated in the dull tints of their plumage is the tone of the desert wild. The well-known case of the ptarmigan is even more extraordinary still. In summer the bird shows a plumage of pearly grey, which conceals it perfectly as it lies on its bed of Scottish heather, mingled with the lichen and its kith and kin. But when the winter snows descend and coat the hillsides with a mantle of white, then a kindly nature still contrives concealment for the ptarmigan in a fresh suit of colour. The pearly greys of the summer are replaced by a plumage of snowy whiteness, and, save for its dark eye, there is little risk of the discovery of the bird by the unwary or unpractised sportsman. The grouse and common partridge are not less perfectly protected. The hues of the grouse match the tints of the heather, and the partridge is almost as difficult to discover—say, in a ploughed field—as the ptarmigan on the hill-side. The birds just mentioned are all *rasorial* birds; that is, they are allied to the type of the common fowl, and are typically ground-livers. Their tints, therefore, assimilate with those of the ground and with ground vegetation; and whatever may be the ultimate philosophy which shows the origin of such harmonies, it is very plain that the utilitarian is bound to read "protection" in every line of the story. Escape from their enemies must be favoured by the correspondence in colour to which we allude. The harmonies of colour present the safest, and therefore the best foil, to the keenness of sight of the eagle, and to the agility of the falcon and its kind. It is different, indeed, with the songsters of the wood and grove. With well-developed powers of flight, and with a close refuge amid the foliage of the wood, the appearance of bright hues and tints in these birds is by no means disadvantageous. Another law—that of the development of colour in relation to sex—has taken precedence of the regulation of colour as a means of protection. If concealment be necessary, nature will teach the art of hiding in other ways than that whereby she contrives to make the partridge face danger with a stillness that almost rivals that of the stones, trustful in the harmony of her plumage that so closely matches her heather bed.

But there are wider fields open to the naturalist's survey of colour and its meanings. Suppose that we peer for a moment into the class of fishes, we shall find the adaptation of colour to surroundings illustrated in a very apt degree. Whoever has tried to spear a sole or flounder, for example, well knows that the excitement of the sport consists in the endeavour to follow out the axiom of Mrs. Glasse, and on the principle which that worthy lady laid down about "first catching your hare," to first catch your flounder. You cautiously and softly paddle out to shallow water in your punt, and you drift over the flat, sandy beach at a depth of from two to three feet. Below, the water is as clear as crystal. Here and there you see a lazy starfish on the march, exerting himself to the utmost, as he slowly extends ray after ray, and crawls at the rate of about a mile a month or so, by aid of his hundreds of sucker-feet. The sand-eels annoy you as they burrow downwards and send up little clouds of dust on your approach; but the flounders you came to spear—where are they? and echo seems but to answer "Where?" But the practised sportsman bids you learn (as in all other sciences and arts) the first lesson—namely, how to see and observe. As your boat creeps along, he points to what seems a mere sandy lump, but in which his keener eye has detected the merest wriggle of a fin. Dash! goes the spear, and up comes a flounder, and as you watch the ground, you see dozens, it may be, of similar sandy patches swimming off in rapid alarm. The flounder's "back"—it is really the *side* of the fish—on which it lies, is white enough, as we know; but the "other side" is as close a representation of a sandy patch as you can see or as you can imagine. Small wonder, then, that in flounder-spearing you experience the difficulties which nature throws in the way of capture through likeness in colour to the animal's surroundings. It is the same with soles, turbot, and with the skates and angel-fishes. Watch the

first flounder you see resting on the sandy bed of the Aquarium-tank, and you will receive ample proof of the truth of the foregoing remarks. And should you chance to see the lazy "monk," or angel-fish, as it lies prone, heavy, and indolent in the highest degree in the flow of its tank, you may again understand something of the value of colour as a means of protection to animal life.

In the case of those "queer fishes," the little sea-horses, or hippocampi, with heads like horses, and with a body which, at large, reminds one most forcibly of some figure from the Heralds' College on a crest, concealment is effected in a slightly different fashion from that prevalent among the soles. Here the body, as a rule, possesses long streamers or fringes that mimic the seaweeds; so that, as the animal reposes, its body may well enough represent a stone, to which are attached fragments of marine vegetation. The Australian sea-horses, which live among red seaweeds, have streamers of that hue attached to their bodies, and the mimicry and imitation of their surroundings are thus very complete. Even their near neighbours, the pipe-fishes, with green bodies, when they fasten themselves to some fixed object, and "loll" in the water, may closely resemble an inert piece of green weed.

Amongst even the highest animals, protective colouring is common. A lion's hue matches the sand, as a tiger's stripes, according to Mr. Wallace, imitate very closely the foliage and trees amidst which it crouches. The camel's coat is sandy like its desert; and the rabbits offer as plain examples as any of the colour harmony in question. The polar bear is white, like the arctic fox in winter dress; and the nocturnal rats and moles are dressed in shades the opposite of the ghost-like hues that become so conspicuous at night.

## THE SOUTH EUROPEAN VOLCANIC SYSTEM.

THERE are reasons for believing that all the volcanoes—and, indeed, all the regions of subterranean disturbance in Southern Europe—belong to a single volcanic system.

It is, indeed, rather difficult to define the exact relation between the different parts of a widely-extended volcanic region. At a first view it seems unreasonable to assert that if eruptions or other forms of disturbance in different places are simultaneous, this must be regarded as evidence that the two places belong to the same volcanian region, while a similar conclusion should be deduced from the fact that quiescence in one spot synchronises with disturbance in another, and *vice versâ*. For instance, it was noticed that during the often-interrupted eruption of Vesuvius in 1868, Etna was more or less disturbed, until at length, as if in sympathy with the Neapolitan volcano, the Sicilian mountain gave vent to enormous streams of molten matter; and this of itself might be considered to afford satisfac tory evidence of the existence of some sort of connection between Etna and Vesuvius. But on the other hand, we are told that when the great earthquake of Calabria took place early in 1783, the inhabitants of Pizzo remarked that the volcano of Stromboli, which is in full view of Pizzo at a distance of about fifty miles, smoked less and threw up a less quantity of heated matter than it had done for several preceding years. Then, again, on the same occasion the great crater of Etna gave out enormous quantities of vapour as the Calabrian earthquake began, while Stromboli seemed exceptionally active as the commotion of the earth in Calabria began to diminish; yet no eruption occurred from either of those great vents during the whole progress of the Calabrian earthquakes. Are we to infer from this, as in one place Sir C. Lyell seems to do, that the volcanic fires of Etna and Stromboli are "very independent of each other," or that the same mutual relation exists between them as between Vesuvius and the volcanoes of the Phlegræan Fields and Ischia, "a violent disturbance in one district serving as a safety-valve to the other?" Lyell completes the latter sentence, by the way, by adding, "both never being in full activity at once." But here it seems to us he loses the full value of the evidence which the various disturbances of the South European volcanic regions have afforded. Rightly viewed, both forms of evidence equally tend to show the inter-dependence, neither showing the independence, of different parts of this great region. If an eruption of Vesuvius begins precisely when an eruption of Etna seems for some cause or other to be checked, or diminished in activity, the circumstance seems in itself to suggest, if it does not prove, that the two vents serve to relieve one and the same volcanic region. If, again, Vesuvius and Etna both burst suddenly into violent eruption at one and the same time, surely that also is evidence that they belong to the same volcanian system.

There is no reason whatever for assuming that because a disturbance in one region may serve as a safety-valve for another, therefore both can never be in full activity at once. Experience has shown repeatedly that two regions of volcanic disturbance, the movements of which sometimes alternate, may at other times be simultaneously active.

But it may perhaps be asked, if this really is so, how can we possibly decide whether any two volcanian regions whatever are connected or distinct? For instance, when Chimborazo is active, Vesuvius is either active or at rest. If Vesuvius is active, the two vents may be said to be working simultaneously; and so the preposterous idea may be adopted that these two volcanoes, though separated by so many thousands of miles, relieve the same region of subterranean disturbance. If, on the other hand, when Chimborazo is active, Vesuvius is at rest, then the same absurd conclusion is derived from the consideration that the action of Chimborazo alternates with that of Vesuvius. In reality, however, plausible though this objection seems, it has no weight. The distinction between the simultaneity or the reverse of the action of Vesuvius and that of Chimborazo, and the associated or alternated efforts by which Vesuvius and Etna, or Etna and Stromboli, or Vesuvius and the Phlegræan Fields, relieve one and the same region of subterranean disturbance, is sufficiently well marked. Compare the dates of the eruptions of Chimborazo and Vesuvius with the actual range of time over which the observations extend, and no connection whatever can be observed between them. Vesuvius must, indeed, be either quiescent or at rest when Chimborazo is in eruption, but Vesuvius never (or only by the merest accident) begins to be disturbed when an eruption of Chimborazo commences; nor has an eruption of Chimborazo ever synchronised, except by the merest accident, with the cessation of an eruption of Vesuvius, or *vice versâ*. But whether we find that an eruption of Vesuvius ceases very soon after an eruption of Etna has begun, or that both volcanoes burst at once, or nearly at once, into eruption, the coincidence of contrasted conditions implies equally with the coincidence of similar conditions, that the two volcanoes are outlets of the same volcanic region.

In other large regions of subterranean activity we notice similar relations—namely, at times periods of well-marked oscillatory disturbance, and at other times periods of simultaneous action. We also find volcanic regions where the outlets are nearly always in action together at

times of disturbance, and other regions where alternations of activity are the only evidence of connection. Thus the Peruvian and Chilian volcanic regions have seldom, if ever, been simultaneously disturbed. A long period of disturbance in the Peruvian region, culminating in the terrible earthquake of Riobamba, was followed by a series of disturbances in the Chilian region, resulting in a permanent elevation of the whole line of coast. Then came disturbances in the Peruvian Andes, including the tremendous earthquake of the year 1868, which so shook the shores of Peru, that the sea wave then generated swept right athwart the Pacific to the shores of New Zealand, Australia, and the Asiatic continent. It is noteworthy that between the Chilian and Peruvian regions of disturbance there is a space in which no volcanic action has ever been observed; precisely as between the oscillating ends of a balance there is a region of comparative quiescence.

It certainly does not seem probable that these signs of disturbance in different parts of the South European volcanic region which have followed or accompanied the eruption of Vesuvius have been free from all connection with the activity of the great Sicilian vent. Certainly earthquakes in Sicily have been associated with the cessation of the outflow of lava from Etna. Nor can there be much doubt that signs of activity shown by Vesuvius after Etna has been in eruption, and *vice versâ*, indicate sympathy between the Neapolitan and Sicilian volcanic regions. It may seem extravagant to associate earthquakes in France or in the British Isles with the activity of the Italian volcanoes; but there are many reasons for believing that such slight earthquakes as do from time to time occur in these more northerly regions depend indirectly on the condition of the South European region of subterranean activity. It is certain that shortly before the great outburst of Vesuvius in 1868, an earthquake more marked in character than usual in this country shook the western parts of England. It is well known, too, that when another part of the great southern region of disturbance was affected, and Lisbon was laid in ruins, the lakes, rivers, and springs of England were disturbed in a remarkable manner; the water of Loch Lomond, for instance, suddenly rose, without apparent cause, against its banks, and then quickly subsided to its usual level.

## THE RELATION OF FOOD TO MUSCULAR WORK.

By Dr. W. B. Carpenter, F.R.S.

PART II.

IT has been shown that whilst Liebig regarded the production of Muscular Energy as an expenditure of the "vital force" of the muscle-substance itself—involving its death and chemical transformation, and requiring *nitrogenous* or tissue-food for its regeneration—Mayer attributed the production of that energy to the oxidation of a portion of the *non-nitrogenous* or respiratory food, regarding the muscular apparatus as the mere instrument by which that oxidation is made to produce Motion in place of Heat.

I shall now give a general account of the experimental inquiries by which Liebig's doctrine has been disproved and that of Mayer established. These have reference to the relation of the *amount of work* done by the body as a whole,—(1) to the relative consumption of the nitrogenous and the non-nitrogenous components of the food; (2) to the amount of Carbonic acid exhaled; and (3) to the amount of Urea passed off:—while (4) another set of experiments upon isolated muscles has demonstrated the dependence of the production of energy upon a change in the substance of the muscle itself, rather than (as Mayer supposed) in the blood passing through it.

1. If a man be carefully "dieted" for a time long enough to determine the amount of *nitrogenous* aliment adequate to repair the ordinary daily "waste" of his tissues, and the amount of *non-nitrogenous* aliment needed to maintain the heat of his body at its normal standard and keep him in healthful exercise, so that his weight remains the same at the end of the experiment as at the beginning—and he is then set to make a great addition to his daily exercise in the shape of walking, pumping, turning a machine, or the like—it is found that his body can be kept up to this extra work by an increase in the supply of *non-nitrogenous* aliment, with such a small addition to the nitrogenous as may suffice to make good the loss produced by the increased "wear and tear" of the machine itself.

2. All observations concur in showing an *immediate* increase in the exhalation of Carbonic acid, capable of being determined with rigorous exactness, when the body, previously at rest, is put into motion. The late Dr. Edward Smith made himself the subject of a long-continued and diversified series of researches upon this point, by the use of a portable gas-meter, through which he could breathe when walking or working at a tread-wheel, as well as when standing still, sitting, or lying: and he found not only that when continually "getting up stairs" on the tread-wheel he exhaled more carbonic acid than when walking, that when walking fast up-hill he exhaled more than when walking slowly on level ground, and that in the latter case he exhaled more than when standing still; but that he exhaled more when standing than when sitting, and more when sitting upright (without support to the back) than when lying so fully supported as not to put forth any muscular effort. Further, he found that when, in walking, he carried a weight even of a few pounds, the exhalation of carbonic acid was sensibly augmented; the increase being still greater when the weight had to be *raised* (as in walking up hill), as well as transported. These results have been confirmed by numerous other experimenters. They are in remarkable harmony with those long previously obtained by Mr. Newport, upon the relative amounts of carbonic acid exhaled by a bee at rest, and a bee "buzzing" under a glass.

3. The employment of more exact methods for the quantitative determination of Urea than that used by Liebig, has shown that he was altogether wrong in asserting that a corresponding increase is produced by muscular exertion in the quantity of that substance eliminated by the kidneys. An experiment which has now become "classical" was performed upon themselves by Professors Fick and Weslicenus in 1866; namely the determination of the respective quantities of urea eliminated by each of them for twelve hours *before*, for eight hours *during*, and for six hours *after* the ascent of the Faulhorn, whose height is about 6,500 feet. They took no nitrogenous food either for seventeen hours before the ascent, during the eight hours of the ascent, or for six hours after the ascent; but then took a good ordinary meal. The mean of the two determinations (between which there was a very close correspondence) gave for the twelve hours before the ascent, 0·62 gramme, being at the rate of 0·052 gramme per hour; for the eight hours of the ascent 0·40 gramme, or at the rate of 0·05 gramme per hour; and the same amount for the six hours following the ascent, being at the rate of 0·066 gramme per hour; while for the twelve hours after the subsequent meal, the mean amount was

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0·48 gramme, or at the rate of 0·04 gramme per hour. There was thus a positive reduction in the amount of urea eliminated, which was probably attributable to the temporary abstinence from nitrogenous aliment; since the results of subsequent observations carried-on for a much longer period upon men going through severe exertion upon an ordinary diet (as those made by Dr. Austin Flint, of New York, upon Weston, the pedestrian, during a five days' walk of 310 miles), show a slight total increase in the elimination, which is fairly attributable to the general "wear and tear" produced by the excessive strain put upon the machine. There is, then, no foundation whatever for the assumption of Liebig, that every exertion of muscular energy involves the death and disintegration of an equivalent amount of muscle-substance.

4. It now appears certain that the chemical change which is the source of Muscular energy occurs in the Muscle itself, not in the stream of Blood that courses through it. For the muscular tissue nourishes itself at the expense, not merely of the *protoplasmic* constituents of the food brought to it by the blood-current, but also of the *saccharine;* and, in addition, takes in oxygen which the red corpuscles of arterial blood bring to it from the lungs. A sort of explosive mixture is thus formed, which is "fired" (so to speak) by the nerve-discharge; a certain quantity of the saccharoid being thus caused to unite with the oxygen contained in the tissue, producing at the same time the *heat* which raises the temperature of the muscle, and the *motor force* exerted in its contraction; while the carbonic acid, which is the product of this oxidation, together with the residual water,* is conveyed away in the return-current of venous blood. But that this "explosion" (the term is only used figuratively, to indicate the *suddenness* of the chemical change, and its excitement by nerve-discharge) does not involve the destruction of the tissues, is evident from the consideration stated by Mayer, that the amount of work done by the heart (which is capable of very exact measurement) would require, if it involved the disintegration of the muscular tissue, a renewal of it at the rate of *a heart per week*. But muscle-substance constitutes no exception to the general rule that every tissue in the body has a term of life of its own; as we see in the rapid waste it undergoes when entirely thrown out of use. And it is the need of renovation thence arising, that gives rise to a demand for *nitrogenous* aliment; this being used to keep the machine, so to speak, in working order, not to serve as its fuel.

Again, it was formerly supposed by physiologists that the conversion of arterial into venous blood (which chiefly consists in the replacement of a portion of its oxygen by carbonic acid) takes place in the capillary network of the system generally; but we now know that it goes on at very diverse rates in different parts, and varies in the same part according to its functional activity. Now, this variation especially shows itself in the blood that passes through the Muscular substance; for when a muscle is at rest, the blood returned by its veins retains almost completely the character of that brought by its arteries; whilst, if the muscle be thrown into contraction by nervous or electric stimulation, the returning blood at once assumes the ordinary venous character—thus showing the dependence of its conversion upon the action of the muscle. We get a corresponding result by experimenting on a separated muscle; that of a cold-blooded animal being most suitable, as longest retaining its vitality when removed from the body. If the muscle of a Frog, placed in a closed chamber exhausted of all save watery vapour, be repeatedly called into contraction by electric stimulation, the chamber is found, after a time, to contain carbonic acid in a quantity proportionate to the number of such contractions, showing that some component of the muscle-substance has undergone oxidation; and since no other product of chemical action is discoverable, it may fairly be concluded that what has been given off from the muscle is part of the saccharoid matter which chemical analysis proves to have been previously stored up in its substance. The union of the saccharoid with the oxygen also stored up in the muscle, produces an amount of energy that can be determined (in the form of units of heat) by the quantity of carbonic acid found in the vessel. This energy, however, may express itself in motion as well as in heat; and the amounts of both may be determined with considerable precision—the former as units of *work* done, the latter as units of *heat* by which the temperature of the muscle is raised. Now, if the units of work be turned into their equivalent units of heat, and the two amounts be added together, they give a total so closely accordant with that deduced from the quantity of carbonic acid produced, as to afford the most striking confirmation of Mayer's prediction:—"Convert into heat," he said, "the mechanical product yielded by an animal in a given time, add thereto the heat directly produced in the body during the same period, and you will have the total quantity of heat* which corresponds to the chemical processes."

To sum up:—The *mechanical working* of the body of a living animal is as directly dependent as its *heating* upon the oxidation of the hydro-carbons of its food; and these may be most economically supplied by non-nitrogenous substances. On the other hand, the mechanism can only be kept in working order by the continual renovation of its substance (its very existence as a living whole involving the continual death and decay of its component parts); and for this renovation a supply of *proteids* is essential, with a certain admixture of *fat* to serve as material for protoplasm.

I have thought it worth while to enter somewhat fully into the particulars of this inquiry, since it affords an excellent example of the truly scientific methods on which Physiology is now being studied, and of the value of the results that are being obtained by their use. It is by such methods alone that the Physical and Chemical actions taking place within the living body can be so determined as to give to Physiology that place among the exact sciences which its earlier cultivators could scarcely venture to hope it would ever attain.

* The *saccharoids* may be regarded as consisting of carbon *plus* the components of water, so that the amount of energy (whether manifested in Heat or in Work) produced by their oxidation, is proportional simply to the quantity of carbonic acid generated. In *oleaginous* compounds, on the other hand, the number of oxygen atoms is not nearly equal to that of the hydrogen atoms; so that their oxidation generates not only carbonic acid, but also water. It is pretty certain that this metamorphosis, like that of saccharoid, takes place in the substance of the muscle, since it must be the chief source of energy in carnivorous animals, whose food contains no saccharoid. But we have no means of distinguishing the water thus generated from that which is otherwise present.

* It would appear from the experiments of Fick upon frog's muscles, that of the whole energy generated by the oxidation process, about five-sevenths show themselves as heat, whilst the other two-sevenths do the mechanical work. In the bodies of Man and other warm-blooded animals, however, the proportion of heat to work is ordinarily much larger; the former constituting about five-sixths of the total energy generated by the oxidation of the food, while the latter is only about one-sixth. Still, considered simply as a contrivance for doing mechanical work, the human body compares favourably with a steam-engine; the very best form of that machine only exerting about one-eighth of the power which is generated by the combustion of the coal it consumes, the remaining seven-eighths being wasted.

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## BRAIN TROUBLES.

IN these days, when the energies of the mind have become more important than those of the body, and when even the health of the body is chiefly of value because of its direct association with the health of the mind, it is well that all who have much brain-work to do should know and understand the symptoms indicating derangement or overwork of mental powers. Of course, in all cases where, through whatever cause, any specific mental malady is in question, the assistance of physicians who have given special attention to cerebral diseases must be obtained. But fortunately with most, even of those who work the brain hardest, no more real occasion arises (whatever some doctors would assure us) for medical advice respecting mind troubles, than commonly arises in the case of corporal troubles among men who pass their days in hard but healthy bodily toils. The saying that every man is either a fool or a physician at forty (thirty would, perhaps, be nearer the mark) may be applied at least as well to the case of the mind as to that of the body. It is as easy for one who is not the fool of the proverb to understand the signs which indicate mind-mischief, and to minister to the mind when out of sorts (not actually diseased), as it is for him to note the signs of bodily ill-health, and apply the remedies which experience has shown him to be appropriate. And here we would note generally, what it is one object of this article to indicate specifically, that the analogy may be carried somewhat further. There are few greater mistakes, so far as the body is concerned, than to imagine every little ailment to be a sign of actual disease, and to have recourse for such slight troubles either to medical advice, or (which may prove more mischievous still) to active medicines or other strong remedies. The physician of the proverb, that is, the man who, not being a fool, has learned to understand his own constitution under ordinary conditions, may be watchful, if he so pleases, of even the slightest indications of ill-health, general or local, so long as such watchfulness does not degenerate into hypochondria. But most of these indications should suggest to him only such changes of diet, exercise, hours of resting, and so forth, as his experience has found to be suitable, and should in the greater number of cases suggest negative rather than positive remedies even of this kind. Many signs of illness, indeed, which obtrude themselves on the attention even of those who watch themselves least in such matters, may far better be dealt with by the patient himself than by the physician. For instance, the present writer has learned to regard severe headaches of a certain type simply as affording evidence that certain articles of food (milk, butter, cheese, and the like) must either be given up altogether for several days, or taken in much-reduced quantity. When this course is followed, he is freed from all such attacks, until after the lapse, perhaps, of two or three months, a headache of this particular kind shows him that he has taken such articles of food in greater quantity than is desirable for one of his constitution. A doctor might prescribe with advantage for the cure of the attack itself, and there can be no reason why a person troubled by some severe attack of headache, muscular rheumatism, or the like, should not obtain from a doctor some active medicine by which to diminish the pain from which he suffers; but it is a far more important matter to ascertain the regimen by which such attacks may be prevented from occurring, and this is a matter which a man (not being the "fool" of the proverb) should manage for himself. Now what is true of bodily troubles is true of mental mischief, short of actual disease, though doctors who have learned, rather late, to leave men a good deal to themselves, so far as the former are concerned, are by no means ready to admit that mental troubles can also for the most part be remedied without calling in the physician. Writers like Forbes Winslow, and others who have dealt with obscure diseases of the mind, have done service in calling attention to certain signs of cerebral mischief which laymen might be apt to overlook; but they insist rather too strongly on these as indicative of actual disease, whereas it is within the experience of thousands that such signs, in the majority of cases, are no more to be regarded as necessarily indicating disease, than a passing feeling of nausea necessarily indicates an approaching fever, or than a pain in the bowels necessarily indicates an approaching attack of Asiatic cholera.

It should also be noted, that much mischief may be caused by suggesting that tricks and failings of the mind, which are quite common, are signs of serious cerebral mischief. Not long after the first edition of Forbes Winslow's treatise on "Obscure Diseases of the Mind" appeared, a friend of the writer's, who had begun to read the book only because of his interest in matters scientific, found that it possessed for him a strange fascination, because nearly all the phenomena mentioned by Winslow as indicative of approaching insanity were such as he had frequently noticed in his own case. Thereafter regarding these symptoms in the light in which they were thus presented, this unfortunate student of cerebral science found himself presently possessed by a strange terror lest the state which Winslow seemed to indicate as a necessary sequel of these familiar signs should be close at hand in his own case. The evil progressed until his mind was really endangered by these mistaken fears; but, fortunately for him (if madness is rightly regarded as the greatest of all evils), a series of misfortunes befell him which for a time altogether withdrew his attention from the mental phenomena which had so excited his fears. For two or three years he had to contend against great pecuniary difficulties, and to endure a series of domestic calamities of no ordinary order. Compelled to withdraw his attention from his own mind, he forgot that, according to the teachings of mental physiologists, he had been fairly on the way towards either mania or idiocy. Four or five years later, chancing to take down Forbes Winslow's book from his library shelves, he read with amusement the passages which had formerly excited his fears. He knew that the mental symptoms graphically described by Winslow still presented themselves from time to time—when, for instance, he was tired or unwell bodily—but he had learned in a very practical way that they are not quite so ominous as the mind-doctors assert. It is indeed possible (perhaps probable, or even certain) that no cases of acute mania may be noticed which have not been preceded by such symptoms; but assuredly these symptoms are not in every case—probably not in one case out of hundreds of thousands—the signs of actual mental disease, nor in one case out of millions followed either by acute mania or by apoplectic seizure, as in the exceptional cases dealt with by Dr. Forbes Winslow.

We propose hereafter briefly to consider some of the signs which show that the mind is temporarily out of order, requiring rest, relaxation, or change of employment. We may in some cases have to enforce the lesson we wish to inculcate by citing cases in which such symptoms have been followed by serious mental disturbance; but we wish at the outset to persuade our readers that, in far the greater number of cases, these signs suggest only the necessity for ordinary precautions, not for medical advice or active remedial measures.

*(To be continued.)*

XUM

# COMETS.

## Part II.

MOST persons know that the name "comet" is derived from the word *coma*, or *hair*, and is applied to celestial objects which appear to have a hairy appendage. Modern astronomers do not, indeed, use the word *coma* in this sense, but draw a distinction between the *coma* and the tail. There can be no doubt, however, that the part now called the comet's tail was that from which these objects derived their name. The word *cometa* or *cometes* is not a lately-formed one; but was used by Cicero, Tibullus, and other ancient writers, and it is worthy of notice that all the names applied to comets by the Romans had a reference to *hairiness—stellæ comantes, crinitæ, concinnatæ*, they are called by Ovid, Pliny, and Cicero. The last term —signifying stars which show a *curled* or *crisped* hairiness —would not be very applicable, by the way, to any comets that have appeared in modern times. The Chinese applied to comets the name *sui*, or "broom."

It might be supposed that the hairy, broom-like, or tail-like appendage, so commonly seen in comets, is really a distinctive feature of these comets. This, however, is far from being the case. A very large number of comets have no visible tails. We refer, of course, principally to telescopic comets; for very few comets which have been conspicuous to the naked eye have wanted this appendage.

The *coma*—in the modern astronomical sense—is never wanting. This term is applied to a misty, hazy light, surrounding on every side a small bright spot, which is termed the *nucleus* of the comet.

When first seen in the telescope, a comet usually presents a small round disc of hazy light, somewhat brighter near the centre. As the comet approaches the sun, the disc lengthens, and, if the comet is to be a tailed one, traces begin to be seen of a streakiness in the comet's light. Gradually a tail is formed, which is turned always *from* the sun (Fig. 1). The tail grows brighter and longer, and the head becomes developed into a coma surrounding a distinctly-marked nucleus. Presently the comet is lost to view through its near approach to the sun. But after awhile it is again seen, sometimes wonderfully changed in aspect through the effects of solar heat. Some comets are brighter and more striking after passing their point of nearest approach to the sun (or *perihelion*) than before; others are quite shorn of their splendour when they re-

Fig. 1.—Changes of a Comet when first seen.

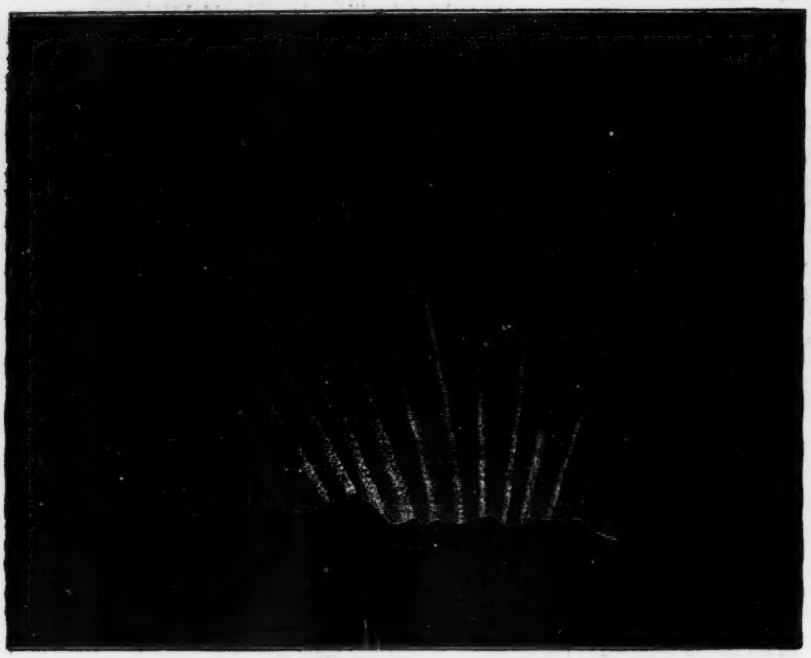

Fig. 2.—The Comet of 1744 (*Cheseaux*).

Fig. 3.—Comet of 1861 (*Newton's*).

appear. The latter was the case with the comet of 1835-36, as we have already seen. On the other hand, the comet of 1861 burst upon us in its full splendour *after perihelion*-passage.

Some comets have more than one tail. One appeared in 1744 which had no less than six tails, symmetrically disposed (if one can trust the pictures handed down to us) in the figure of a half-opened fan (Fig. 2). Others have presented a yet more peculiar appearance, having, besides a tail in the usual position, a second "unconformable" tail, at right-angles to the first, or inclined to it at some incongruous, out-of-the-way angle—for instance, in one case, one hundred and sixty degrees. Sometimes the peculiarity is presented of a perfectly dark gap separating the tail from the head. More commonly a dark space is seen behind the head, but on each side of this space the light from the head is continued so as to form a bright border on each side of the tail.

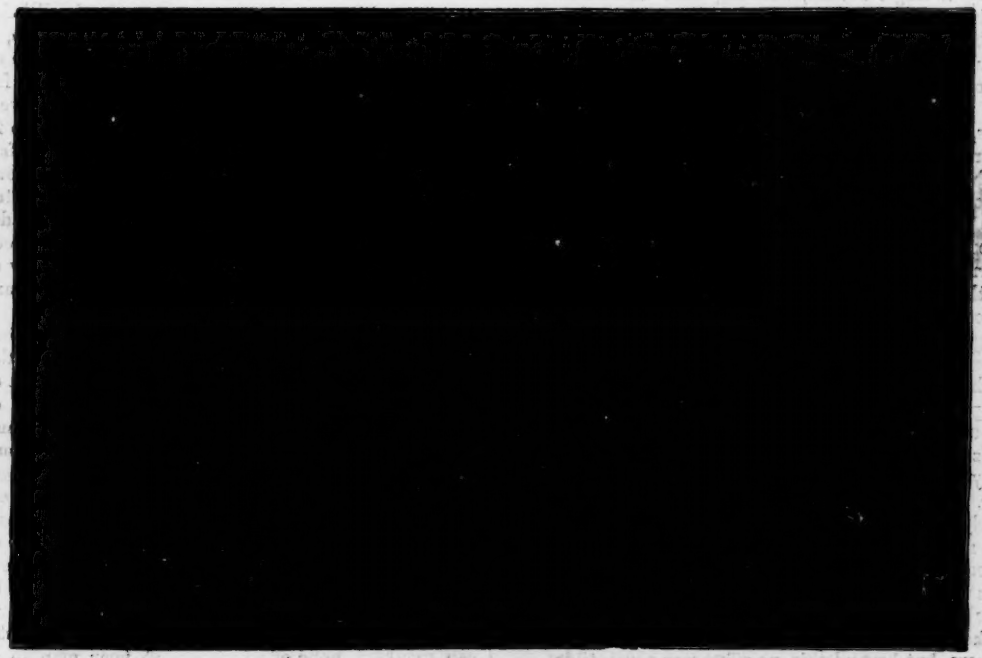

Fig. 4.—Comet of 1843.

As a comet approaches the sun, we have seen that a change takes place in the appearance of the coma and nucleus, and that in some instances a tail is generated. The process actually observed is generally this: in the forward part of the nucleus a turbulent action is seen to be in progress, leading to the propulsion towards the sun of jets or streams of misty-looking matter. Sometimes a regular cap or envelope is seen to be projected in this manner towards the sun, or even a set of envelopes one within the other. The matter thus thrown off is not suffered to pass very far from the nucleus towards the sun, but is swept away, as fast as formed, in the contrary direction. If the funnel of a steam-engine were directed forwards, instead of upwards, then the appearance presented by the emitted steam, as the engine rushed on (against a hurricane, suppose, to make the illustration more perfect) would exemplify the process which seems to be taking place around the front of the nucleus, and far behind it, as the matter formed is continually swept away *from* the sun. The same sun which attracts the nucleus seems to repulse the emitted matter with inconceivable energy. Consider for a moment what took place with Newton's comet in 1680-81 (Fig. 3). When this comet was about as far off from the sun as our earth (ninety million miles) it began to throw out a tail. But the comet was going far nearer to the sun than this. Onwards it rushed under the powerful influence of the sun's attraction, until it had crossed the whole space of ninety million miles, making—almost in a straight line—for a point only one hundred and thirty thousand miles from the sun's surface. In four weeks it traversed that vast distance, and then, suddenly (in a few hours) sweeping half round the sun, started on its return journey. But note this: as it approached the sun, the comet had thrown out a tail continually increasing in length, and pointing *back* almost along the orbit; then the comet is lost to sight for a few days, and when it is next seen returning rapidly from the sun, it has a tail pointing *forwards* (a tail which *must* be a different one, since—as Herschel says—"we cannot conceive a comet's tail to be brandished round like a stick"), and ninety million miles in length. So that, whereas the comet, already moving with a tremendous acquired velocity, had taken four weeks in traversing a distance of ninety millions of miles under the sun's *attraction*, the matter composing the tail had been thrown to the same enormous distance by the sun's *repulsion* in scarcely one-tenth part of the time, possibly (for the tail was formed when first seen) in a few hours!

The comet of 1843 (Fig. 4) was yet more remarkable for the dimensions of its tail and for its close approach to the sun. The tail of this comet stretched half-way across the sky in March, 1843. Its real length was two hundred million miles *at least*, for the end of the tail was lost to view through the excessive faintness of its light. So near did this comet pass to the sun, that many astronomers did not expect ever to see the comet again.

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But—after all but grazing the sun—sweeping round him at a distance of less than one-tenth of his diameter, the comet escaped and passed back again into space.

When we see the tail of a comet occupying a volume thousands of times greater than that of the sun itself, the question naturally suggests itself, "how does it happen that so vast a body can sweep through the solar system without deranging the motion of every planet?" Conceding even an extreme tenuity to the substance composing so vast a volume, one would still expect its mass to be tremendous. For instance, if we supposed the whole mass of the tail of the comet of 1843 to consist of hydrogen gas (the lightest substance known to us), yet even then the mass of the tail would have largely exceeded that of the sun. Every planet would have been dragged from its orbit by so vast a mass passing so near. We know, on the contrary, that no such effects were produced. The length of our year did not change by a single second, showing that our earth had been neither hastened nor retarded in its steady motion round the sun. Thus we are forced to admit that the actual substance of the comet was inconceivably rare. A jar-full of air would probably have outweighed hundreds of cubic miles of that vast appendage which blazed across our skies, to the terror of the ignorant and superstitious.

The dread of the possible evils which might accrue if the earth encountered a comet will possibly be diminished by the consideration of the extreme tenuity of these objects. But the feeling may still remain, that influences, other than those due to mere weight or mass, might be exerted upon terrestrial races in the course of such an encounter. The subtle breath of some mephitic vapour might penetrate our atmosphere, and, if it did not bring immediate destruction, might leave dire forms of plague and pestilence to work their evil will upon the human race. This fear is not, perhaps, wholly unreasonable, though—as will presently appear—the positive information we now have does *not* favour the supposition that the tail, at any rate, of a comet is likely to exercise such destructive effects. And it is only the tails of comets that we have much chance of meeting. On account of their enormous volumes, it is not so utterly improbable that we should encounter them as that we should meet the comparatively minute nuclei. In fact, there is reason for supposing that the earth actually did pass through the tail of the comet of 1861. At about the hour when it was calculated that the encounter should have taken place, a strange *auroral glare* was seen in the atmosphere, but beyond this, no effect was perceptible.

## INTELLIGENCE IN ANIMALS.

FEW of the questions raised in Darwin's "Descent of Man" are at once more difficult to deal with satisfactorily, or more important in their bearing on the subject of that volume, than the question how far animals possess mental powers akin to those of man. It is somewhat singular, we may remark in passing, that Darwin and Huxley, whose views in some respects are so similar, and who are regarded by the general public as standing side by side in their advocacy of the theory of the relationship of man to the lower animals, should seem to uphold almost exactly opposite opinions respecting the cerebral qualities of animals—one maintaining that in some cases animals reason, while the other (if we rightly apprehend what Huxley has said about animal automatism) will scarcely allow that animals even possess consciousness.

We propose here to consider some cases in which animals have seemed to reason. The importance of the subject will be recognised if we remember Darwin's admission that, had no organic being except man possessed any mental power, or if man's powers had been of a wholly different nature from those of the lower animals, we should never have been able to convince ourselves that our high faculties had been gradually developed. Darwin expresses his belief that there is no fundamental difference of this kind. "We must also admit," he says, "that there is a much wider interval in mental power between one of the lowest fishes, as a lamprey or a lancelet, and one of the higher apes, than between an ape and a man; yet this immense interval is filled up by numberless gradations." But this has not been so generally admitted, despite the evidence advanced by Darwin, as might have been expected. The feeling is still commonly entertained that a distinction exists between the mental qualities of the cleverest ape and the dullest and stupidest savage, which is utterly unlike any that exists among animals. In this essay we shall have to consider cases in which rats, cats, dogs, &c.,—animals all inferior in mental faculties, though not all in equal degree, to the more intelligent apes—have acted in ways which seem to imply reasoning. We shall treat these cases rather from the point of view of an opponent of Darwin's thesis above quoted than of a supporter, endeavouring in every case to find explanations not involving the exercise of reasoning faculties. But we must admit at the outset, that we find ourselves led to precisely the conclusion which he has indicated.

In the first place, we must recall to our reader's recollection those instances which have been selected by Darwin as so satisfactory, that in his opinion any one not convinced by them would not be convinced by anything that he could add.

Rengger states, says Darwin, "that when he first gave eggs to his monkeys, they smashed them, and thus lost much of their contents; afterwards they gently hit one end against some hard body, and picked off the bits of shell with their fingers. After cutting themselves only once with any sharp tool, they would not touch it again, or would handle it with the greatest care. Lumps of sugar were often given them wrapped up in paper, and Rengger sometimes put a live wasp in the paper, so that in hastily unfolding it they got stung" (the tenderness of some of these students of science towards animals is quite touching). "After this had once happened, they always first held the packet to their ears, to detect any movement within." These were not monkeys of the higher orders, but American monkeys, none of which are so near man in cerebral development as the orang, the chimpanzee, the gibbon, or the gorilla. The next cases relate to the dog, and are important, first, because two independent observers give evidence in the same direction; and secondly, because the action of the dogs can hardly be explained as resulting from the modification of an instinct. "Mr. Colquhoun winged two wild ducks, which fell on the opposite sides of a stream; his retriever tried to bring over both at once, but could not succeed; she then, though never before known to ruffle a feather, deliberately killed one, brought over the other, and returned for the dead bird. Colonel Hutchinson relates that two partridges were shot at once, one being killed, the other wounded; the latter ran away, and was caught by the retriever, who on his return came across the dead bird. 'She stopped, evidently greatly puzzled, and after one or two trials, finding she could not take it up without permitting the escape of the winged bird, she considered a moment, then deliberately murdered it' (the winged bird), 'by giving it a severe crunch, and afterwards brought away both together. This was the only known instance of her having wilfully injured any game.'

"Here," proceeds Darwin, "we have reasoning, though not quite perfect, for the retriever might have brought the wounded bird first, and then returned for the dead one, as in the case of the two wild ducks." If the dog had followed the wiser course, it would not have been quite so clear as in the actual case that he had reasoned, though the pause for consideration after an attempt to take both together, would have gone far to suggest that explanation. But the action of the dog in killing the bird seems quite decisive, because such an act was entirely opposed to the instincts of the breed and to the training which retrievers receive.

To these cases Darwin adds the statement that "the mulateers in South America say, 'I will not give you the mule whose step is easiest,' but *la mas racional*—the one that reasons best"; on which Humboldt has remarked, "this popular expression, dictated by long experience, combats the system of animated machines better, perhaps, than all the arguments of speculative philosophy." Here, although Darwin only quotes Humboldt, he manifestly expresses his own view, and we find him opposed in a very definite manner to the theory of Kepler, afterwards supported by Descartes, and recently advocated by Huxley and others, that animals are automata, not possessing consciousness (or at anyrate that this theory is admissible).

The next case to be considered is one which was described a year or two since in *Nature*. It was not one which in reality demonstrated, or even strongly suggested, the exercise of reasoning faculties by animals. We quote it, however, because it illustrates well the mistakes into which want of care may lead the student of our subject. During the cold weather of last January, the writer of the letter in question put bread on the window-sills of his drawing-room for the benefit of the birds. These, finding food there, were constantly fluttering about the windows. "One day a large water-rat was seen on the window-sill, helping himself to the bread. In order to reach the window he had to climb to a height of about 13 ft.; this he did by the help of a shrub trained against the wall. Neither instinct nor experience," proceeds the correspondent of *Nature*, "will easily account for his conduct, since he never found food there before. If neither experience nor instinct, what, save reason, led him? His action seems to have been the result of no small observation and reasoning. He seems to have said to himself: I observe the birds are thronging the window all day; they would not be there for naught; it may be they find there something to eat; if so, perhaps I, too, might find there something I should like. I shall try." The way in which this story is told singularly illustrates the difficulty which we, as speaking animals, find in understanding how a process of reasoning can be carried on without the imagined use of words. Probably few men whose mental powers have been well trained carry on a process of pure ratiocination, without clothing with words the thoughts successively suggested to their minds. It almost seems to a mind thus accustomed to reason with a verbal accompaniment (audible to the mind's ear only) that any mental process not thus accompanied must be to some degree instinctive, and any actions resulting from such a process automatic. But it is certain that even the most intellectual sometimes act in a manner which, if noticed in an animal, would suggest the exercise of reasoning power, not only without putting their thoughts into mental language, but without, in reality, noting what they are doing. However, the point to be specially noticed about the above story is that the narrator overlooks the most obvious, and probably the true, explanation of the rat's behaviour. The rat could not see the food, but most probably he could smell it. If so, his adventuring up the wall to get it was not the result of reasoning, or, at least, not necessarily so, for that was the shortest path to the much-needed food. Possibly the birds themselves may have been an attraction to him. Certainly the case is not one which compels us to believe that water-rats reason.

This objection was so well urged, in company with other points necessary to be considered in such inquiries, by a German writer, Herr H. T. Finck, that we quote his remarks almost in full. "Before we ascribe to a rat such complicated reasoning powers," Herr Finck remarks, "it is necessary to ask if there is no other simpler way of accounting for the phenomena. I think there is. It is well-known that different species of animals vary greatly in the acuteness of their senses. To man, sight is the most important sense, and the same is true of many other animals and most birds. The cat is a representative of another smaller class of animals, whose most perfect organ of sense is the ear; while the dog lives in a field of sensatives, the most important of which are contributed by the sense of smell." This point, as dogs afford many of the most striking illustrations of reasoning, or of what looks like reasoning, in animals, must be carefully remembered. Few are aware, we believe, how imperfect a sense is sight with all dogs, as compared with our own sense of sight. We believe that there could not be cited a single instance tending to show that a dog has been able to see as well as a very short-sighted man would, while in the great majority of cases, it can be shown by a few easily-tried experiments that dogs scarcely see at all in the true sense of the word. Our sense of smell is probably not more completely inferior to the same sense with dogs, than is their sense of sight to ours. To return, however, to Herr Finck. After pointing out that the rat belongs to the class of animals who are guided by the sense of smell, he says, "It is evident, therefore, that the water-rat in question was led to the window-sill by his nose, which in his case was a more trustworthy guide than his eyes would have been. I do not wish to deny, by any means, that animals have reasoning powers. On the contrary, I am convinced that human and brute intellect differ only in degree, not in kind. But what we have to guard against is not to ascribe [he obviously means the reverse, that we are to guard against ascribing] to animals reasoning powers of a higher type than is consistent with the development of their brain, especially when the actions which seem to postulate such powers can be readily accounted for by simply bearing in mind the extraordinary acuteness of one or more of their senses. We are altogether too prone to judge the intellectual life of animals by the human standard—to imagine that the eye is everywhere, as with us, the leading source of knowledge. The neglect of the important *rôle* which the sense of smell plays in animal life has been particularly fruitful of errors in philosophical speculation. It has, among other things, helped to give a longer basis of life to the old theory of instinct, regarded as a mysterious power of nature." In passing, we may remark that at the very beginning of our own life the sense of smell is stronger and more useful than the sense of sight; as though during those first few days, before the eyes acquire power to recognise objects or to do much more than to distinguish light from darkness, we belonged for the time being to that inferior class of animals with whom the predominant sense is that of smell. In that part, also, of their lives, human beings seem so far to resemble the lower animals that their actions appear to be governed by instinct solely. In reality, probably, a sense of smell much keener then than during the subsequent years which alone we can remember, governs the actions in the same way, though not so obviously, as sight governs them in most of the actions of later years.

XUM

## BIRDS WITH TEETH.

IN the year 1861 a feather was found in a slab of lithographic stone from Solenhofen, which Hermann von Meyer assigned to an animal as yet not otherwise known, which he called *Archæopteryx lithographica*. Later in the same year, a large portion of the skeleton of Archæopteryx was discovered in the same formation. There were impressions of feathers radiating fanwise from each of the forelimbs. But Prof. Andreas Wagner, in a report to the Royal Academy of Sciences in Munich, expressed the opinion that the creature was not a bird, but a reptile, whose natural covering presented a deceptive resemblance to feathers. He called it the *Griphosaurus*, which (considering he had not seen the fossil remains) was very obliging on his part. Von Meyer, however, regarded the impressions as representing real feathers, belonging to the same animal as the feather he had already discovered. The fossil was secured for the British Museum in 1862. It is contained in two slabs of Solenhofen limestone; one representing the surface of tidal mud on which the bird lay at the time of its death, the other the layer deposited over the dead body. The lower slab shows the impressions of the tail, wings, and parts of the skeleton. The right shoulder-blade and upper arm (wing), as well as both the forearms, are well preserved. The head, the neck, and the backbone are wanting. Two of the digits of the wing (wing fingers we may call them) are free, and armed with sharp claws or recurved spurs. The right lower limb is well preserved, consisting of the thigh-bone, the tibia or larger lower leg-bone, and the tarso-metatarsal bones, or bones of the upper foot. To the metatarsus, four toes are articulated, one hind-toe and three fore-toes, which are jointed as with birds, and armed with strong recurved claws. "The foot," says Mr. Woodward, from whose description the above has in the main been taken, "agrees well with that of a true perching bird, but from the fanwise and rounded arrangement of the wing-feathers, it would appear to have been a bird of feeble flight."

Without entering further into the peculiarities of this creature, we note that while a few naturalists were doubtful as to its being really a bird, the majority were very confident that it was so. Professor Owen, in particular, pointed out that in one respect in which it differed most from modern birds it resembles the embryonic bird. Its tail-bones diminished gradually to the last, whereas in modern birds, the last vertebra of the tail is almost always the largest. But, said Owen, "All birds in their embryonic state exhibit the caudal vertebra distinct, and in part of the series [of embryonic changes] gradually decreasing in size to the pointed end one." The two-fingered and free condition of the wing-hand, that is of what corresponds to the hand in the bird's fore-limbs (which Owen pleasantly described as the biunguiculate and less confluent condition of the manus), he did not account for in the same way as a feature of an embryonic bird; but in some modern species the forward wing finger supports a claw, and the Screamer has two claws. All who at that time examined the fossil agreed that in all probability the creature had a beak like a bird.

But Mr. John Evans noticed somewhat later (besides a rounded mass which he took for part of the brain-pan, with a cast of the brain) what he regarded as a fossil jaw, on the slab on which lies the fossil body of the bird. It had been supposed to be the beak of Archæopteryx, but "great was my surprise," writes Mr. Evans, "when I detected along its right-hand margin, towards the apex, the distinct impression in the slab of four teeth still attached to it. The teeth themselves remain adhering to the counterpart, and are easily recognised by the lustre of their enamel." The teeth are thus described by Mr. Woodward. "The three which remain in a vertical position with regard to the jaw are about one-tenth of an inch long, and at intervals of about one-fifth of an inch. They consist of a slightly tapering flattened enamelled crown, about a twenty-fifth of an inch in width, and obtusely pointed, set upon what is apparently a more bony base, which widens out suddenly into a semi-elliptical form, so that at the line of attachment to the jaw the base of one tooth comes in contact with that of the next. So sudden and extensive is this widening of the base, that at first it gave me the impression that the teeth were tricuspidate, with the middle cusp far longer than the others. The front tooth of the four, which slopes forward from the rest, and is rather smaller than the others, shows little, if any, similar enlargement of its base. Of the fifth, which lies across the base of the foremost of the four, only a part is visible. There appears to be a well-defined line at the base of the teeth along their junction with the jaw, but I can offer no opinion as to the method of their attachment."

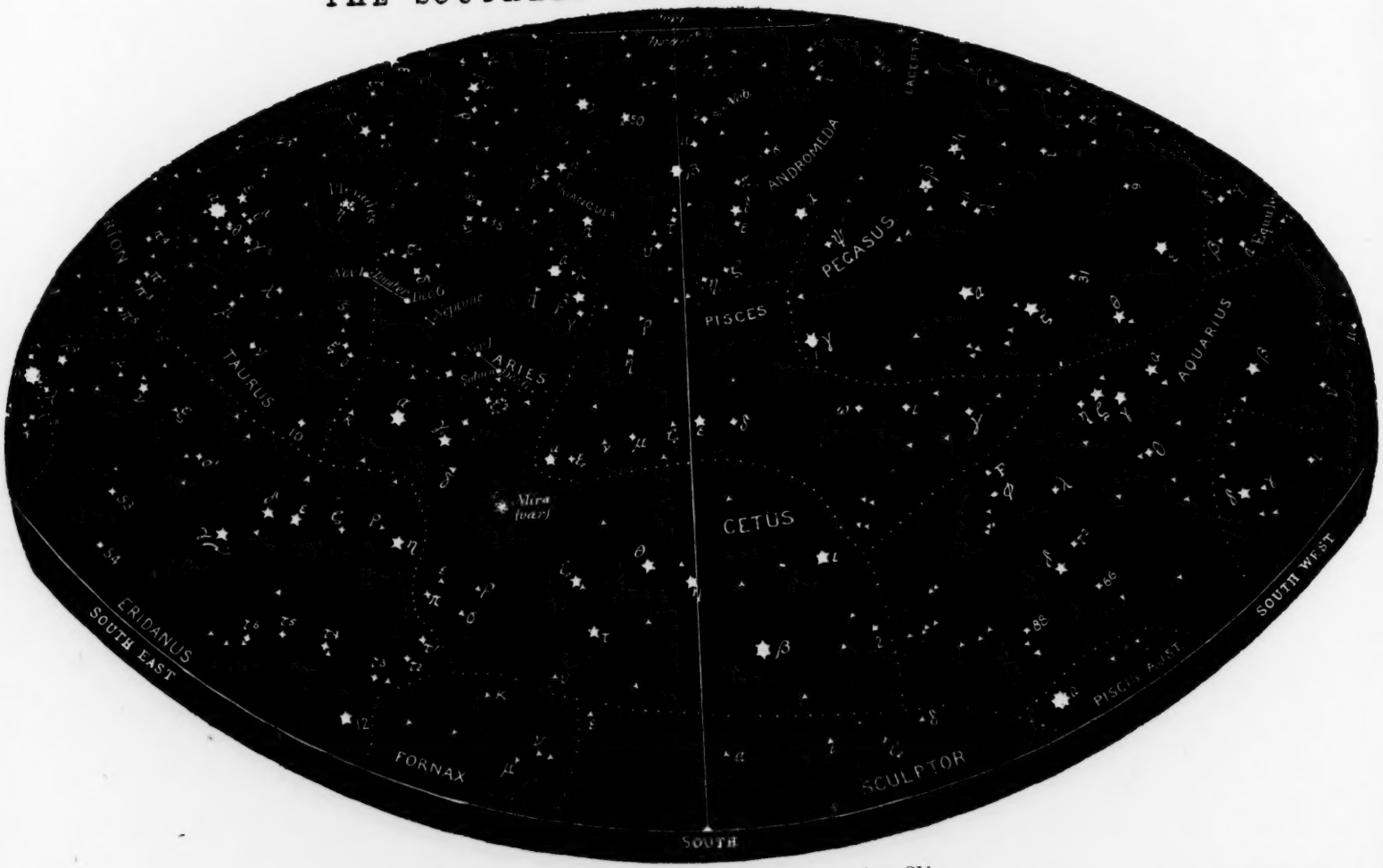
It seemed so unlikely when the above description was written that a jaw armed with teeth could belong to a creature manifestly bird-like, that many supposed the jaw belonged to some fish, though the jaws and teeth of fossil fishes from the same bed were found to be unlike this. Hermann von Meyer, referring to the drawings sent to him by Mr. Woodward, said that he knew of no tooth of the kind in the lithographic stone; nor were the teeth like those of Pterodactyles (the great reptiles with bat-like wings). "An arming of the jaw with teeth would contradict the view of the Archæopteryx being a bird or an embryonic* form of bird. But, after all," he proceeds, "I do not believe that God formed his creatures after the systems devised by our philosophical wisdom. Of the classes of birds and reptiles, as we define them, the Creator knows nothing, and just as little of a prototype or of a constant embryonic condition of the bird which might be recognised in the Archæopteryx. The Archæopteryx is, of its kind, just as perfect as other creatures, and if we are not able to include this fossil animal in our system, our shortsightedness is alone to blame."

Probably the theory that the Archæopteryx had teeth would still be regarded as little better than an assumption, had not other and more complete evidence been obtained, Professor Marsh discovered two fossil birds in the cretaceous shale of Kansas, which had well-developed teeth in both jaws. Of one of these birds — the *Ichthyornis Dispar* — "the teeth were quite numerous," Marsh wrote in *Silliman's Journal* for October, 1872, "and implanted in distinct sockets. They were small, compressed, and pointed, and all of those preserved are similar. Those in the lower jaw number about twenty in each ramus" (that is, on each side), "and all more or less inclined backwards. The series extend over the entire margin of the dentary" (or tooth-bearing) "bone, the front teeth being very near the extremity. The maxillary teeth" (those in the upper jaw, that is) "appear to have been equally numerous, and essentially the same as those in the mandible. The skull was of moderate size, and the eyes were placed well forward. The lower jaws are long and slender. . . . The jaws were apparently not encased in a horny sheath."

* The word embryonic is here used with reference to the species, not to the individual. It signifies a form which creatures of the species presented before the type of the species had become, as it were, distinct and established. Traces of such past forms of a species are recognisable in the embryonic development of later representatives of the species.

[*Continued on page* 33.

# THE SOUTHERN SKIES IN NOVEMBER.




*This Map shows the position of the stars in the Southern Skies:* —

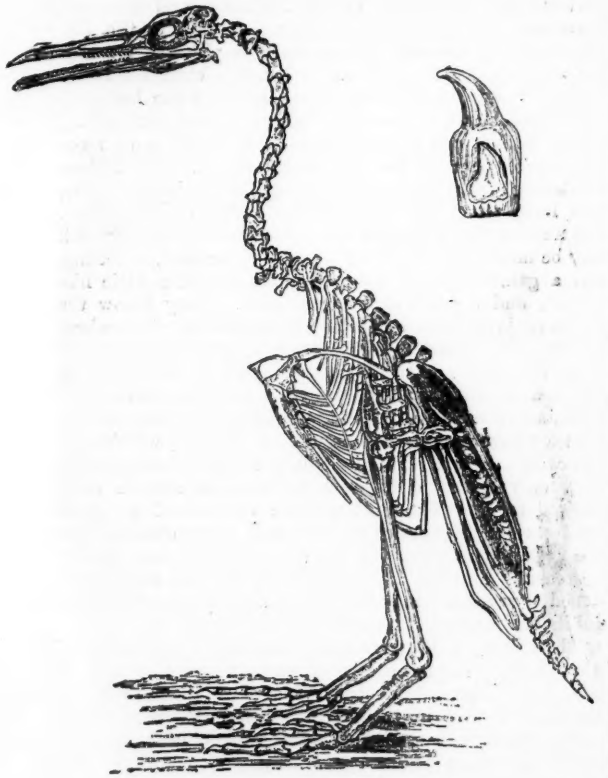
On October 30, at 11½ o'clock. | On November 7, at 11 o'clock. | On November 14, at 10½ o'clock. | On November 22, at 10 o'clock. | On November 29, at 9½ o'clock.
On November 3, at 11¼ o'clock. | On November 10, at 10¾ o'clock. | On November 18, at 10¼ o'clock. | On November 25, at 9¾ o'clock. | On December 3, at 9¼ o'clock.

In this map, as in the map for last week, the movements of some of the planets are illustrated. Saturn and Neptune have already passed opposition, or their place where they are due south at midnight. Jupiter comes to opposition on Sunday Nov. 13th. He is now nearly at his brightest.

*** Stars of the first magnitude are shown with eight points, stars of the second with six, of the third with five of the fourth with four, of the fifth with three.

*Continued from page* 30.]
The shoulder-blades and the bones of the wings and legs were all of the true bird-like type. The breast-bone had a prominent keel. The wings were large in proportion to the legs. The wing-bones corresponding to the hand in man, were united as in ordinary birds. The bones of the hinder extremities resembled those in swimming birds. The bird was about as large as a pigeon. The species was carnivorous, and probably aquatic. Professor Marsh called the other form discovered by him *Apatornis celer*.

Later, Professor Marsh announced that having re-examined another fossil bird—a large diving bird nearly six feet high, found in the same cretaceous formation as the *Ichthyornis*, he found that it also had *teeth in both jaws, not in sockets*, like the Ichthyornis, *but in grooves*, as in *Ichthyosaurus*, the great lizard-formed marine reptile.



The skeleton of this toothed bird is pictured in our illustration. Prof. Marsh called it the *Hesperornis Regalis*. Before the discovery of teeth, Prof. Marsh had unhesitatingly classed the Hesperornis as a gigantic diver, though recognising peculiarities of structure. But recently, in a Monograph on the Extinct Toothed Birds of North America, he called attention to its resemblance in certain respects to the Ostrich. He says that if these characters are to be "regarded as evidence of real affinity, the *Hesperornis* would be essentially a gigantic swimming ostrich." Professor Huxley, on the other hand, says that the bird is "in a great many respects astonishingly like an existing diver or grebe—so like it, indeed, that had this skeleton been found in a museum, I suppose—if the head had not been known—it would have been placed in the same general group as the divers and grebes of the present day."

The teeth seem to have been admirably adapted to aid a diving bird (like a grebe) in catching its slippery prey. In the *Odontopteryx toliapicus* of Owen, the bony denticles were inclined at a considerable angle, but with the points forward, yet Professor Owen concluded that even such projections (they could not properly be called teeth) must have greatly assisted the bird in holding captured fish. In the existing bird, the *Merganser serrator*, the tooth-like serrations are inclined with the points backwards. These serrations, however, were not teeth, but merely tooth-like extensions of the horny covering of the beak. The teeth of the *Ichthyornis* and *Hesperornis*, as is shown by the smaller figure (showing a tooth, and, within it, a tooth forming to take its place) were unmistakably teeth. It does not take away from their dental character that they were set in a groove in *Hesperornis* and *Archæopteryx*, instead of in separate sockets, as in higher-toothed races and in *Ichthyornis*.

It should be added that Professor Marsh has examined the specimen of *Archæopteryx* in the British Museum, and fully satisfied himself that it belongs to the class of toothed birds. "The teeth seen on the same slab with this specimen agree so closely with the teeth of *Hesperornis*, that" he "identified them at once as those of birds, and not fishes."

He describes the leading characters of the ancestral bird in the following terms:—"In the generalised form to which we must look for the ancestral type of the class of birds, we should expect to find the following characters: Teeth in grooves; vertebræ biconcave" (that is, the bones of the backbone shaped somewhat as we see them in fish); "breastbone without a keel; tail longer than the body; bones of the hand and wrist, as also those of the foot, free; the bones of the pelvis separate; the sacrum" (or hind bone of the pelvis) "formed of two vertebræ; four or more toes directed forward; feathers rudimentary or imperfect."

If we consider the circumstances under which, according to the theory of evolution, the race of birds came into existence, we can understand that the ancestral creatures whence birds are descended presented many features in which they were not only unlike the birds of our time, but unlike any other race of existing animals. Were they not also, in all probability, very unlike each other? Probably there were much wider differences among the various orders of animals, which included all the ancestry of the modern bird, at the time when first *any* of the characteristics now regarded as avian first existed, than there are now among all the orders of existing birds. This certainly appears from the evidence obtained, not only respecting toothed birds, but also respecting those bird-like animals of which Huxley and others have shown that they were closely akin to reptiles—were, in fact, biped reptiles. We believe that the same holds with every species now existing, even with man—that, for instance, if we could have brought before us in rapid review all those creatures from which the human race of our time has descended (taking only those which belonged to one particular epoch, before man, specialised as we now find him, existed), we should not only find a far wider range of difference among these creatures than among the human races of the present day, but a wider range of difference than even exists between men and apes. There are *à priori* reasons for this view as regards the human race; but, apart from these, the evidence collected by Mivart in his work, "Men and Apes," while not, we think, available to show that there is no kinship between the Simian and Human races, seems only explicable on the assumption that the Simian ancestors of man differed widely *inter se*.

XUM

## THE FIJI ISLANDS.*

THE ideas generally entertained respecting the Fiji Islands and their inhabitants are not such as to encourage the idea that life to white men would be very pleasant there. Probably most persons, who have not followed the changes which have recently taken place in this important group of islands, suppose that the Fijians are still, as they used to be considered, the most barbarous of all the Polynesians, addicted frightfully to cannibalism, and little changed from those who, as Herbert Spencer puts it, possessed such "extreme loyalty," that if the king willed it, a Fijian cheerfully stood unbound to be knocked on the head. The days are passed, however, when a Fijian king could register by a row of many hundred stones the number of human victims he had eaten. The Conservative Fijian sighs in vain for the good old times when the king's will reigned supreme. A visitor has now only to take with him, as Mr. Horne did, a circular letter of introduction written in Fijian to all the chiefs, to find himself a welcome guest at (instead of upon) their hospitable tables. "In each village some one, generally the schoolmaster, 'teacher,' or native clergyman was found, who could read and explain the letter to the people, who were at all times attentive listeners." The Sunday schools are well attended, and most of the rising generation of Fijians can do something in the way of reading, writing, and ciphering. In fact, with a few guides and an interpreter, a little sugar, tea, coffee, and biscuits, mats for sleeping upon, a rug or so, and a mosquito net, the visitor can enjoy himself immensely in the Fiji Islands, as Mr. Horne's work shows in almost every page.

While the cool weather lasts, Europeans in Fiji can wear with comfort clothing adapted to an English summer; "indeed, at this season, the weather is delightful, finer than the best summer weather in England." In the hot weather, it is true, the heat is oppressive, while storms of thunder and heavy rain are more frequent than pleasant. With reference to the rainfall, by the way, which even for a tropical country is very heavy, Mr. Horne notes a circumstance of considerable interest. "Previous to and during 1861-2 the low hills around Levuka were thickly wooded. Since that time the woods have been cut down, and the number of days on which rain falls has been reduced from 256, the average for 1861-2, to 149, the average for 1865-6 and 1876-7. It would seem that the number of showers diminished simultaneously with the cutting of the trees. The average rainfall has not been much diminished, however, and with an annual rainfall of 118 inches the Fijians may be well satisfied, especially as the rain falls most abundantly during the warm or summer season, when vegetation most requires it. It was absolutely necessary, moreover, to clear the forest region, for the thick woods afforded shelter to the mountaineers, who, on several occasions, appeared in large numbers, and threatened to sack the town and murder the white settlers. "These marauders came from Lasoni, in the centre of Ovalan, just across the mountains from Levuka; stole down upon the town, plundered the goods of the settlers, and then made off into the woods, where it was useless and dangerous to follow them." Unfortunately, since the woods were cleared the rain falls more torrentially than before, and carrying away the loose soil on the surface, where the ground is steep, does great damage to both soil and vegetation.

The Fiji Islands number in all 255, having an entire area of about 7,403 square miles, or about 738,350 acres. The largest island of the group, Vili Levu, has an area of 4,112 square miles, while the next in size, Vau na Levu, has an area of 2,432½ square miles. The others are all much smaller. About eighty of the islands are inhabited, the white population being about 2,000 (in 1874), the natives numbering about 140,500. As regards communication with the outside world, Fiji is not badly off. Twenty-four hours after the arrival of the mail from San Francisco at Sydney, a fine steamer of 1,000 to 1,500 tons leaves for Levuka, the voyage occupying seven or eight days. The steamer remains at Levuka nearly a week, and leaves with the mails for England in time for them to be transhipped to one of the Peninsular and Oriental steamers at Sydney. From Melbourne there is direct steam communication with Suva and Levuka once every five weeks. There is also regular steam communication between Levuka and Auckland (New Zealand), and between Levuka and the Friendly Islands. A visit to the Fiji Islands during the cool season would be pleasant for any one who enjoys change of scene; but it is clear from Mr. Horne's book that the naturalist (especially the botanist) would find such a visit at once interesting and profitable.

The natives are hospitable, as also (which is of more importance, perhaps) are the white settlers. The Fijians are daring sailors, and good customers to the boat-builders, who have taken the place of the native canoe-builders. The natives play a number of athletic games, among which may be mentioned throwing the *tinika*, or reed, wrestling, and a game which is something like tennis, a little like cricket, and a great deal like skittles. They throw the tinika (an oval-shaped piece of wood about four inches long and two in diameter at the thickest part) a distance of about 300 yards, or thrice as far as our best cricketers can throw a cricket-ball. The natives are subject to elephantiasis, and consider their children neither strong nor healthy till they have experienced an ulcerous disease, which they call *coko*. A kind of ophthalmia is not uncommon, but it lasts only a few days, both natives and settlers being subject to it. The natives have succumbed in great numbers to epidemics of measles, and many consider that the population has, in consequence, become greatly decreased. But Mr. Horne considers that the many abandoned "patches" may indicate rather a change in the habits of the people than a diminished population.

The animals indigenous to Fiji are bats, flying foxes, and a small rat. The ten species of snakes found there are all harmless. Pigs, introduced from Tonga (where Cook left them), run wild in the forests. There are wild ducks, snipe, sand-pipers, wild pigeons, and beautiful golden orange doves, as well as parrots or parroquets. Whales and porpoises abound in the seas round the group, which swarm with many kinds of fish, edible and otherwise. Sharks abound in the seas, and travel long distances from the sea to the deep pools in the rivers, which must render them less pleasant to bathers than they otherwise would be.

Mr. Horne's book is full of interesting facts, and though it has been specially written in response to an official invitation, it will be found very pleasant reading. There is a copious index—indeed, the index is a little too copious, a passing word in the text being, in many cases, all that is found to bear on a carefully-paged index heading. The facts gathered together here are the fruits of a year of faithful and laborious research.

* "A Year in Fiji: an inquiry into the Botanical, Agricultural, and Economical resources of the Colony." By J. Horne, F.L.S., &c., Director of Woods and Forests, and Botanical Gardens, Mauritius.

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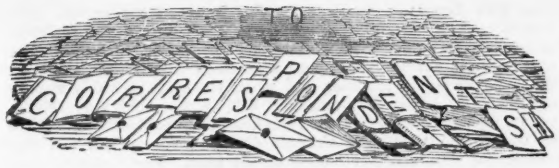


## Letters to the Editor.

*[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. He requests that all communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]*

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**** All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.*

*All Letters or Queries to the Editor which require attention in the current issue of* KNOWLEDGE, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

## Our Correspondence Columns.

### THE 1-INCH MAP OF THE ORDNANCE SURVEY.

[4]—From time to time we hear expressions of admiration at the beauty and fidelity of those maps of Great Britain and Ireland, based upon that great Trigonometrical Survey, which really began in 1784. In fact, an "Ordnance Map" is with many people a kind of synonym for all that is topographically accurate in the shape of cartography. I venture, however, to express my belief that praise is bestowed upon these maps—be it observed that I am now speaking only of those on the scale of 1 inch to a mile—I say, I believe it will be found that praise is bestowed upon them in the inverse ratio of their employment for purposes where minute accuracy is essential. I have said that the survey began in 1784, because it was in that year that the Triangulation for correcting the Observatories of Greenwich and Paris was commenced by the measurement by General Roy of his famous base of 27,404 feet on Hounslow Heath. I believe, however, that what is now known as the Ordnance Survey had its beginning in 1791, when proceedings were initiated by the remeasurement of the base of which I have just spoken, by Colonel Williams, Captain Mudge, and Mr. Dalby. As an illustration of the manner in which, up to a pretty recent period, the maps on which I am commenting were produced, I may here say that one purchased within a comparatively few years lies before me as I write, and bears upon the upper part of it the legend, "Published 1st February, 1813, by Lieut.-Colonel Mudge, Tower." Now, it is abundantly evident from this, that an old plate must have been worked upon and patched up year after year, alterations being engraved somewhere about the place in which they had occurred; but, as I shall immediately show, they seem to have been put in merely by eye, so erroneous are a large proportion of the minor details when tested with minute precision. To give a single illustration: the mark + stands for a church upon these Maps; but a query as to whether the intersection of the arms of the cross gives the site of the spire (the conspicuous part from which it might well be supposed that all measurements would be made) is always met at the head office at Southampton by the reply that the cross is merely a conventional sign, and does not represent any part of the church in particular! Another stock answer, too, to complainants at head-quarters is that distortion arises from shrinkage in drying the maps: such shrinkage being unequal, and dependent upon the manner in which the maps are hung up to dry after printing. Whence it would appear that after between 70 and 80 years' practice and experience, the combined science of the entire staff at Southampton is unequal to provide for the equable shrinking of a sheet of damp paper. I have been led into these remarks by a recent piece of personal experience, in the shape of the attempted identification of all the chief objects visible from a height, by the aid of a 1-in. Ordnance Map and a 6-in. transit theodolite. One observation will suffice to illustrate the ground of my complaint. It is that of a large and conspicuous church-tower, 8¼ miles from the place of observation; the angle between which and the meridian differed 54′, as measured by the instrument and on the map; 54′, 8¼ miles off, representing, it is almost needless to add, about 706·7 feet, or 235½ yards. The fact seems to be that, while the greater triangles have been surveyed with all the refinements of Geodetical science, the filling in of the details has been done "anyhow." I have, I verily believe, seen quite as accurate plans made by pacing, and with an ordinary prismatic compass. Such of the 25-in. maps as I have examined really do seem to afford remarkable examples of painstaking correctness; and I am informed that those on the 6-in. scale are equally praiseworthy. Moreover, I learn that a new series of 1-in. maps, reduced from this 6-in. survey, are in the course of publication. If this be so, we may live in hope that we shall some day be in possession of a series of portable maps, vieing in accuracy with those issued by the German and other Continental Governments. Meanwhile, let no one go into a shop to purchase one of the present 1-in. Ordnance Maps under the impression that he will receive a rigidly correct chart of the region professed to be represented: because he will not.

A FELLOW OF THE ROYAL ASTRONOMICAL SOCIETY.

### IS THE SUN HOT?

[5]—Will you permit me to remark that "Anti-Guebre's" letter (No. 1., p. 15) is in some respects a repetition of an article of mine entitled "The Astronomy of the Future," which appeared in *Fraser's Magazine*, Nov., 1876, since published in a volume, "Pith," in which I endeavoured to maintain that, in spite of the revelations of the spectroscope, the theory that the sun is incandescent is wholly untenable and improbable. My reasons for thinking so are given at length in the paper mentioned; but perhaps it may be as well to repeat here that if we start with the supposition that the sun is a gigantic galvanic battery, there can be no more reason for believing it to be red-hot, than there is for assuming that the battery we use in our laboratory is a sort of furnace.

Light and heat are surely phenomenal products, caused by magnetic and electrical forces, in a state of intense activity, acting upon atmospheric conditions; so that we are at perfect liberty to maintain that Mercury need not be any warmer or more illuminated than is our earth or Jupiter. The sun may possess the power of producing the phenomena of incandescence, without itself being incandescent.

The inflammatory action apparent on the face of the sun may be merely the chemical conversion of substance into force; and if we could see the working of a dilute acid on the surface of the metallic plates in a galvanic battery, we should probably discover on a minute scale a corresponding commotion to that which is so conspicuous on the sun.

As stars differ from each other in their material composition, it is only natural that the revelations they make of themselves in the spectroscope should be also constitutionally different.—Yours, &c.,

NEWTON CROSLAND.

[6]—You wish for more reasons in favour of the sun's being a cold body. Why is it that, although we have reflected sunlight from the moon, we do not see that light on its way to that body? Surely there would be a broad flood of effulgence along the heavens.

TYRO.

### INFLUENCE OF SEX ON MIND.

[7]—Permit me to take exception to the title of the article "Are Women Inferior to Men?" To hold that woman's mind is, like her body, naturally weaker than man's, does not imply absolute inferiority. Overwhelming evidence to prove woman intellectually weaker than man can be classed under five heads:—1. *Anatomical.*—Head-forms, shape of skull, size of brain. 2. *Physiological.*—Woman is always, more or less, an invalid. Hence, if supposed equal to man in intelligence, she is heavily handicapped by her physical organisation. Sex must influence mind. 3. *Historical.*—If the sexes are equal in mind, why has man's intellectual work so far surpassed that of woman? Why was not the alleged sexual mental equality asserted and proved long ago? 4. *Daily Experience* that women cannot argue, and never see more than one side of any question. 5. Woman's *Superior Instinct;* a decisive proof that she has *less reason* than man.

The subject is highly interesting and important, as determining woman's proper sphere and education. If permitted, I would gladly place my views concisely before your readers.

*Nov. 5.* Yours, &c., J. MCGRIGOR ALLAN.

[Certainly.—ED.]

### THE NOVEMBER METEORS.

[8]—In your first number you invite correspondence, and I therefore make no apology for writing and suggesting that, as your publication is intended for beginners in science, it might be an advantage

if, in your next number, it were pointed out at what hour, and in what direction, intending observers should look out for the November meteors—that is, as proposed in the current number, use "plain untechnical language"; and may I further suggest that (making use of your own two typical words), if you write about small-faced or long-headed men, you would *combine with them* the technical word: *e.g.*, "the race was dolichocephalic, *i.e.*, long-headed." By this means your readers will understand the technical terms, without referring to their lexicons, when they come across them in more advanced books.

*London, Nov. 9, 1881.* A. T. C.

[The meteors of November 13–14 will hardly be seen this year. If seen at all, it will be after midnight, and they may equally well be looked for in all parts of the sky.—ED.]

## THE EARTH'S INCLINATION.

[9]—It may seem a ridiculous question to ask you to answer, but I can find no satisfaction in scientific primers. Will you kindly tell me, simply and clearly, the reason of this:—The earth's axis is at an inclination of 23° (?) to the sun. Very good. Now suppose the northern hemisphere be nearer that body, if the earth should spin around itself and the sun, why, when it gets on the opposite side, will the northern hemisphere be turned from it? Does the bottom of the body move out [? move nearer], while the top recedes in its annual revolution? TYRO.

## SPEED OF AMERICAN ICE-YACHTS.

[10]—With reference to your reply to my query at p. 16, I must confess that while I see the reasoning I had supposed conclusive is not so in reality, yet I fail to understand how there can be any driving-force on a vessel driven by the wind when her velocity is equal to that of the wind. It seems to me the observed fact that an ice-yacht travels faster than the wind can only be explained by supposing that the wind comes occasionally in blasts much stronger than the average force of the wind. A fierce blast might give an ice-yacht a velocity much greater than the average velocity of the wind, and owing to the slight frictional resistance, she might retain that velocity with very little loss until the next blast came.

UPSILON.

[11]—An explanation of the great velocity attained by ice-yachts is given in the August number of *Scribner's Magazine*. It runs as follows:—An ice-yacht makes "the greatest speed on that course in which she meets with the least air friction, receives the strongest push of the wind in a forward direction, and yet does not lose the wind too much by her own speed. This course is running free, with the wind on the quarter, or about one hundred and thirty-five degrees off her course. Suppose the boat heads north-west, while the wind blows from the west. Now, her speed diagonally across the wind causes her to receive the wind on the beam, as if it blew from the north-west. She practically has a wind on the beam; this offers but little air friction against her forward motion. The running-friction is so slight that the boat keeps her way, the direction of the sail-push is sufficiently forward to be advantageous, and, lastly, her diagonal course, partly with and partly across the wind, saves her from losing too much of the wind's force by her own speed. Suppose that a twenty-knot breeze blows from B to C, and that she heads towards D, while the wind, represented by the arrow A, blows in a given time to C, it carries the boat with it in nearly the same time; but as she heads diagonally across the wind, she is obliged to run the long distance from B to D, while the wind blows only from B to C; she therefore beats the wind. Her speed is limited only by the loss or change of the wind through the effects of her own velocity. The greatest velocity of an ice-yacht is not recorded, because her finest runs occur either at unexpected moments, or when she sails over unmeasured distances. But the time over short and long courses has often been taken. The distance from New Hamburg to Poughkeepsie is over seven miles. The *Snowflake* ran this course in seven minutes. This is the quickest time on record; but many winters the trip has been made in from nine to ten minutes. This speed is attained with a stiff breeze on the beam or on the quarter, and when the ice is tolerably smooth and clear of impassable cracks. But an ice-yacht very seldom runs a straight course for even a mile. Various obstructions have to be avoided; the wind changes direction very often, and also comes and goes in fitful puffs over the hills. The consequence is that she makes a very crooked course at very uneven speed; she goes more than seven miles, and sails at her full speed during much less than seven minutes. Probably she flies at times from eighty to one hundred miles an hour. The speed of an ice-yacht, in working to windward, which is her poorest course, is from ten to fifteen miles an hour, against an eight or ten-knot breeze."

YACHTSMAN.

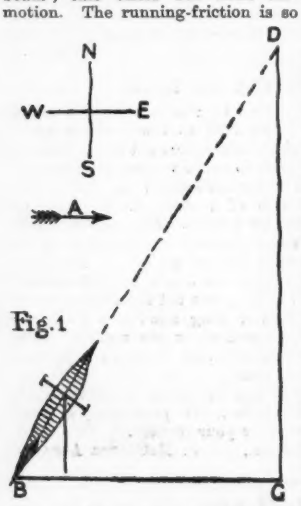

Fig.1

[Thanks to "Yachtsman" for his interesting extract. I do not think the explanation, however, is strictly satisfactory. What we want to have explained is just what the explanation takes for granted, namely, *why* the ship runs the longer distance BD, while the wind has gone over the shorter distance BC. Any account which simply says that she does, or that she is obliged to, can hardly be regarded as an explanation. When we consider, too, that with every change of her velocity there is a change in the direction in which the sail-push acts, of which the above so-called explanation takes no account, it must be admitted that it is open, as an explanation, to objection. The explanation referred to by "Upsilon," which I gave in the *Newcastle Weekly Chronicle*, ran somewhat on this wise:—

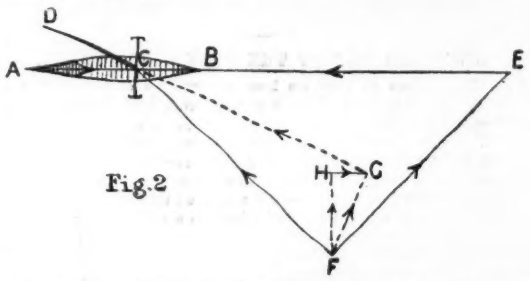

Fig.2

Let AB be the centre line of the yacht (bow at B), CD her sail. Let CE represent the yacht's velocity at any moment, the effect of which is, of course, equivalent to the existence of a wind blowing in direction EC, and having its velocity represented by EC. Let the actual wind be in direction FE (which would be, if the yacht were at rest, a wind on her quarter), and represented in velocity by FE. Then, by the principle known as the "triangle of forces," we know that the effective wind comes in direction FC, and with a velocity represented by FC. Now, we must resolve this into the two velocities FG, perpendicular to the sail, and GC parallel to it; of course the resolved part GC corresponds to a wind which could have no effect whatever, and therefore may be neglected. What remains is a wind blowing with velocity FG square to the sail. This wind we resolve in turn unto two, one HG, parallel to the yacht's course, the other FH, perpendicular to it. The last has no effect in urging on the yacht, only inproducing lee way. The effective driving wind with velocity HG remains, and unless the accelerating force of this wind is counterbalanced by frictional resistance, it will increase the yacht's velocity, which already exceeds that of the wind.—ED.]

## ARE MEN'S HEADS SMALLER THAN OF YORE?

[12]—In a letter to your contemporary *Nature*, Mr. W. B. Kesteven states that in the hat trade the opinion is prevalent that within the last thirty or forty years the size of men's heads has diminished. "The following statement," he says, "has been given to me by a hatter, whose name has attained a pre-eminence lasting more than a generation." "Five-and-thirty years ago, when I was a young man, we used to purchase hats for retail trade in the following ratio:—

| | |
|---|---|
| Sizes | 21—21½—22—22½—23—23½ inches |
| Relative number | 0— 1 — 2— 4 — 3— 1 |

At the present time," he adds, "I am selling hats in this ratio:—

| | |
|---|---|
| Sizes | 21—21½—22—22½—23—23½ inches |
| Relative number | 3— 4 — 3— 1 — 1— 0" |

A manufacturer writes:—"I should say that heads generally are two sizes less than at the time you refer to. A head of more than twenty-four inches' circumference is now quite a rarity, whilst we

make thousands of hats for heads with a circumference of about twenty-one inches." "I have received similar statements," Mr. Kesteven says, "from other members of the trade, both wholesale and retail. The statement comes to me not only from men of experience in the trade, but from men of intelligence and observation exercised beyond the limits of the shop or the factory. It is, I am informed, extensively believed among hatters; it may, nevertheless, be merely a general impression. The diminution, it is said, is observed mostly among grooms and men of that class in the social scale. If this be really the case, the change should be noticeable also among soldiers. The diminution is possibly more apparent than real, and may be traceable to alteration in the style of hair-cutting, or of wearing the hat. It has been suggested to me that men of the present generation have from birth smaller heads, dependent upon an alteration in the dimensions of the female pelvis, in consequence of modern fashion in dress. Of this opinion, however, I obtain no confirmation from eminent obstetricians of whom I have made inquiries. The statement, then, as it stands, is wanting in explanation, and calls for further investigation." Mr. Kesteven quotes the reply sent him by Professor Flower to his question as to the statement made by the hatters, "that men's heads were smaller than they were twenty years ago":—

"Before drawing any important conclusion from such a statement, it would be necessary to know much about the authority upon which it is made. Who, for instance, are the hatters that make it? Do all hatters concur in the same statement? Is it a mere general impression, or is it founded upon actual arithmetical data? Does it refer to any particular class of men, and does it refer to the same class of men? If it should be true, may it not arise from some change of fashion, if only founded upon the size of the hat, and not of the head) other even than the one you suggest of hair being worn shorter—such as hats being worn more on the top of the head than formerly (in old-fashioned prints one sees the hat well down over the ears, which is certainly not the case now), or, perhaps, hats of the kind specified being now worn by a different (perhaps lower) class of the community, or by younger people? All these questions must be considered, and, perhaps, other sources of error eliminated which may not occur at first, before the statement can be accepted. If the evidence of the statement appears to bear investigation, it would be well worth while following it up, as, if true, it would be one of the most remarkable facts with which I am acquainted, that in the space of twenty years a material diminution in the average size of the heads of the same population had taken place—a fact so contrary to all theory and to all experience."

Professor Flower's opinion seems to me very much to the point. I may note, in addition, that the different material of which hats were made thirty years ago may have something to do with the supposed change. Those who remember the heavy beavers of that period will hardly doubt, I think, that they must have been worn more loosely-fitting than the lighter hats of the present time.

Can any readers of KNOWLEDGE throw light on this subject? Considering that the hope of the future lies much in our growing men with larger heads than now, it would be a serious matter were the hatters right.

Are grooms and men of that kind drawn now from the same classes as of old? May not the best of those classes now seek better employment? Or may not emigration have had something to do with the supposed change?—Yours, &c.,

CEREBRUM.

[The question raised by Mr. Kesteven seems to us of considerable interest, though it is utterly unlikely that within so short a time, any change, such as hatters suppose, can really have taken place in the size of men's heads—even if, which is almost as unlikely, any change in the direction suggested is going on at all. We may mention one circumstance, which, however, would hardly affect grooms. Wigs were certainly more commonly worn thirty years ago than now, and wigs in those days were wigs indeed. The average size of hats must have been quite appreciably greater in those times on that account alone, we should imagine. It is, however, really true that hats of 23¼ inches are no longer kept in stock? We should have supposed, from our own observation, that in any good hat-shop a hat of 24 inches could generally be obtained. This leads us to consider another point. Possibly hatters measured heads differently in former times than at present. If they measured round the head then, instead of taking, as now, the two diameters of the cranial oval, they would certainly have had a higher average for the circuit of the head. Any one who has examined the head-shapes in American hat-shops will know that nine heads out of ten are quite irregularly shaped. We have seen some having an outline more like a long oblong than the oval which a well-shaped head should have. But taking the case of a regular oval (or egg-shape), or even a truly elliptic head, the true circumference would be somewhat greater than that inferred from the hatter's reckoning. Take, for instance, a head section having diameters 6 and 7; then, if I remember rightly, the hatter would call the circumference 3 times 6½ inches + 1 inch (*i.e.*, an inch more than three times the mean between the two diameters), or 20½ inches. Now the actual circumference would be in the case of an ellipse—

$$7\,\pi\left[1-\tfrac{1}{4}\cdot\frac{13}{49}-\frac{1}{64}\,\frac{(13)^2}{(49)^2}\&c.\right]$$

$$=\frac{3{\cdot}1416}{21952}\times 143303$$

or 20·58 inches; that is, nearly a tenth of an inch longer. In the case of an oval shape the difference would be about a tenth and a half, while, in the case of an irregular head, it would be fully a quarter of an inch. Where the section of the head is long (dolichocephalic), the difference between the estimated and the measured circumference would be much greater.—ED.]

## THE FIFTEEN PUZZLE.

[13]—I am told that in a magazine article which appeared some time since, you have attempted to show that there are positions in the Fifteen Puzzle from which the won position can never be obtained. As I believe that the won position can be obtained from any position whatever, including that in which the numbers 13, 15, 14 appear in that order on the last line, I should like to know how the reverse has, in your opinion, been demonstrated.—Boss.

[I thought the Fifteen Puzzle was dead, and hoped I had had some share in killing the time-absorbing monster. (It is an excellent puzzle, by-the-way, except when the puzzled ones try to do impossible things with it.) I have no doubt "Boss" has succeeded in obtaining, from the losing position he names, or others of the same class, what he regards as a won position. For instance, he may have obtained the arrangement—

| | | | |
|---|---|---|---|
| | 1 | 2 | 3 |
| 4 | 5 | 6 | 7 |
| 8 | 9 | 10 | 11 |
| 12 | 13 | 14 | 15 |

or some other, which seems as satisfactory to him as the true won position. But he cannot have obtained this last. The proof would occupy too much space to be given here. But "Boss" may try this. Taking any positions at random, let him take the fifteen numbers one after another as they occur, and for each let him count how many come after it which ought to precede it (running along the lines in the way in which we read the lines of a book, or as the numbers run in the won position). Let him add together all the numbers he thus obtains, and call the result the "total displacement." This number will be either odd or even. The vacant square will be either on an odd line (the first or third) or on an even line (the second or fourth), or, for convenience of expression, say the vacant square will be either odd or even. Now, he will find that if the "total displacement" and vacant square are both odd or both even, so they will remain after any change he may make by sliding a block, after two such changes, after three, in fine, after any number of legitimate sliding changes. If the "total displacement" is odd and the vacant square even, or *vice versâ*, so will they be after any number of legitimate sliding changes. ("Boss" will readily see the *raison d'être* of this, after examining a few cases.) No amount of changes, then, will cause the "total displacement" and the vacant square to be both even or both odd, unless they were so at the outset. As they have to be both even in the won position (for which the total displacement is 0, an even number, and the vacant square on the fourth, an even line, whereas, when the last line runs 13, 15, 14, the total displacement is odd and the vacant square even) no amount of changing will bring the losing position, mentioned by "Boss," to the true won position.

In the article referred to I showed that what, as I have above said, will be found on trial in any given cases, must be universally true. I also showed, in a part of the article which most readers found rather tough reading (there were several misprints, too, the article having been written when I was in Australia), that from any position any other of the same class, either losing or winning, can be attained. As there are more than ten millions of each kind, it is not wonderful that the proof of this general proposition was not altogether simple.

It is singular to think that though probably not fewer than twenty millions of persons tried the Boss Puzzle, probably not a

hundredth of the multitudinous positions of which the puzzle admits were occupied among all the positions (set up, attained, or passed through) in the thousands of millions of trials those millions made.

If "Boss" is not content, let him try the easier task of bringing eight blocks from the position shown in Fig. 2 to that shown in Fig. 3.

| | | |
|---|---|---|
| 1 | 2 | 3 |
| 4 | 5 | 6 |
| 8 | 7 | |

Fig. 2.

| | | |
|---|---|---|
| 1 | 2 | 3 |
| 4 | 5 | 6 |
| 7 | 8 | |

Fig. 3.

This is a less complicated task than that of the original "Boss" puzzle, for instead of more than 10,000,000, there are only 181,440 losing positions in a nine-square puzzle. Or he might try to change from

| | | |
|---|---|---|
| 1 | 2 | 3 |
| 5 | 4 | |

to

| | | |
|---|---|---|
| 1 | 2 | 3 |
| 4 | 5 | |

There are only 360 losing positions in a six-square puzzle. Or finally from

| | |
|---|---|
| 1 | |
| 3 | 2 |

to

| | |
|---|---|
| 1 | 2 |
| 3 | |

, where he has only twelve losing positions to trouble him. He will see at once that the last task is an impossible one; but he may rest assured it is not more completely so than the others, and it wastes much less time.—Ed.]

## Queries.

[1]—Ultima Thule.—One often finds the expression *Ultima Thule* used as if indicating the remotest known regions of the earth. Where in classical authors is this expression used, and in what sense?—Alpha Sigma.

[2]—A Fifteen Puzzle.—Most of your readers are no doubt familiar with the puzzle how to send out fifteen school-girls walking, three and three, so that during seven successive walks no two of them shall be more than once in the same set of three. Is there any mathematical treatment of this puzzle corresponding to the mathematical treatment of permutations and combinations? It seems to me that there should be. For instance, one might begin by showing that nine persons could walk out three and three (with the same restriction) for four successive days, and then apply the method of demonstrative induction to show that if 3 $(2n-1)$ persons can be arranged to walk out in the required manner on $(3n-2)$ different days, then also 3 $(2n \times 1)$ persons may be arranged in the required way on $(3n \times 1)$ different days. It will be obvious that the number of persons must be of the form 3 $(2n \pm 1)$; that is, three times some odd number. The successive numbers to be dealt with are, therefore, 3, 9, 15, 21, &c., and the number of days in which the three can go out in the required way are respectively 1, 4, 7, 10, &c.—Rusticus.

[3]—Flight of Birds.—Is it the case or not, that the flight of birds is greatly aided by the presence of warm, and, therefore, light air in the air passages of the bones?—Aërial.

[4]—The Earth's Inclination.—In diagrams explaining the season's differences, the figure of the earth is shown to rotate on an oblique axis. Were it to rotate on an upright axis, would not perpetual summer reign at the poles, with greater heat at the equator?—Moonstruck.

[5]—Hot Winds, Cause of?—The difficulty is as to the force from front or back which will overbalance the tendency to *rise* over the hot area. Take the case of a Mediterranean sirocco, why is there not rather an *in*rush to the hot sandy plains?—L. S.

[6]—Lying To.—Wanted, diagram of forces when a ship lies to, 1° under sale, 2° under steam.—L.S.

[7]—Flying Bridge.—Wanted, diagram of forces, arrangement of rubber, position of ropes, when the vessel has passed the middle of the river.—L. S.

[8]—Volume of Sphere.—Is there any simple way of showing that the volume of a sphere is two-thirds that of the enclosing cylinder?—Archimedean.

[9] Sun-dial.—Can any of the readers of Knowledge describe any method of constructing a sun-dial by which the error arising from the shadow not being sharp may be got rid of?—Solar Time.

[10] The Zoetrope.—In the zoetrope we get a series of pictures of a moving body, showing it in a certain number of positions from among the infinite number that it passes through in accomplishing the movement illustrated. Can any one explain how it is that the impression conveyed to the mind by this incomplete series of views is that of continuous motion? It seems to me that we ought to recognise a certain jerkiness in the apparent movement. But in well-arranged zoetropic illustrations no jerkiness can be noticed.—Zulu.

A Cargo of Human Bones.—New York, Sept. 14. A special from London says:—A great sensation was caused at Bristol by the discovery of a cargo of three hundred tons of human bones being discharged there to the order of a local firm engaged in manufacturing manure. The bones were shipped from Rodosto, at Constantinople, and are supposed to be the remains principally of the defenders of Plevna. There are complete limbs among the horrible cargo, and in some cases the hair still adheres to the skulls. Peter Cooper says it is a common thing among the British to buy human bones. "In fact, they will take all they can get at any time and from any part of the world. They use them for manuring their lands. I have often heard it said that England was manured with bones taken from the battle-field of Waterloo. There is no finer to be had."—*New York Herald.*

Science in the Police Court.*—At Bow-street last week, Mr. Waddy, Q.C., attended before Sir James Ingham, with Mr. Besley and Mr. Bernard Coleridge, to apply for a summons under the Vivisection Act, against an eminent professor of science. Special reference was made to the subsections providing for a licence to be granted to any one practising vivisection, and for the administration of some anæsthetic of sufficient power to prevent the animal feeling any pain during the experiment. If pain was likely to continue after the effect of the anæsthetic had ceased, or any serious injury was inflicted on the animal after the experiment, it was to be killed before it had recovered from the effects of the anæsthetic. When it was desired to extend the experiments for a lengthened period, it was necessary to procure a certificate authorising the extension of the experiments, and the summons was asked on the ground that this provision in the Act had not been complied with. In support of the application the learned counsel read extracts from a report in the *Lancet* on cerebral localisation, the subject having been brought under the notice of the recent International Medical Congress. One of the speakers was Professor Goltz, who had experimented upon two dogs by exposing the surface of the animals' brains, and washing away large portions of the substance by subjecting it to the action of a powerful stream of water. With reference to this experiment, Professor Ferrier has expressed his views upon the subject and gave details of some experiments he had made upon two monkeys. These animals had been operated upon some months previously, definite motor paralysis being procured in one, and in the other absolute and perfect deafness. The animals subjected to these experiments were produced by each of the professors, Professor Goltz asserting that he had removed the greater part of both hemispheres, including all the supposed motor and sensory areas. That the operative procedures to which the animals had been subjected had been extensive was quite obvious upon examination of the skulls, large gaps in the continuity of the upper and external walls of which were felt. Saving some clumsiness in its movements, one of the dogs showed but little signs of injury. It appeared possessed of considerable intelligence, and certainly did not suggest to the onlookers that it was a dog demented. In startling contrast were the two monkeys exhibited by Professor Ferrier. One of these had been operated upon in the middle of January, the left motor area having been destroyed. There had resulted from the operation right-sided hemiplegia, with conjugate deviation of eyes and head. Facial paralysis was at first well marked, but ceased after a fortnight. From the first there had been paralysis of the right leg, though the animal was able to lift it up. Its arms it had never been able to use. Lately rigidity of the muscles of the paralysed limbs had been coming on. The other monkey, as a consequence of paralysis of its auditory centres, was apparently entirely unaffected by loud noises, as by the firing of percussion-caps in close proximity to its head.—From the *Times.*

* The report would be somewhat clearer if it were not taken throughout for granted that the readers must necessarily be medical students. Why should not paralysis of the right side be written, instead of right-sided hemiplegia? wryneck and squinting for "conjugate deviation of eyes and head?" The "motor and sensory areas" are, of course, those parts of the brain which are supposed to regulate respectively the movements and the sensation.

## UNHEALTHY HOUSES.*

IN an interesting lecture at the London Institution, Prof. F. de Chaumont mentions the following causes of disease:—

(1) Want of renewal of air in our rooms.

(2) The pollution of the house air by admixture with sewer emanations.

(3) The contamination of our water supply.

He says that could arrangements be effectually carried out in our dwellings for removing these causes of disease, certain much-dreaded maladies might disappear altogether. "But we must further remember," he proceeds, "that it is not death alone we have to dread, terrible as its effects often are in a household. For every case, we have to bear in mind, argues a considerable number of cases of illness, which, even if recovered from, may leave consequences behind them sufficient to affect the health of a lifetime, and to diminish the power of the sufferer for the work he has to do. The duration of illness alone is often a serious break in the life of a professional man, artisan, or labourer—a break which in some cases may mean the difference between comfort and penury, or between a successful career and a struggle for existence." "I think I shall be understating the case," he says, "when I say that each case of death argues about a dozen cases of illness, although the number is somewhat less in the severer diseases, such as enteric fever, which is fatal in one out of six cases, and diphtheria, which kills one out of three." Taking even these, however, Prof. de Chaumont shows that in London alone from 50,000 to 60,000 weeks of productive labour are totally lost to the community from illness due almost entirely to the unhealthiness of our houses.

The lecture was delivered for the purpose of bringing before the public the subject of sanitary assurance, and to advocate the cause of the Sanitary Assurance Association, founded in November, 1880. The objects of the Association are as follow:—

"The practical application of Sanitary Science generally, and especially the encouragement and development of proper systems of house-drainage, water supply, and ventilation.

"The examination, inspection of, and reporting on houses and buildings, or plans of houses and buildings, either erected, in course of erection, or to be erected, as regards their sanitary arrangements; the supervision by the officers of the Association of any work done, or to be done, by or on behalf of the members of or subscribers to the Association in connection with the sanitary condition of such houses or buildings; and the granting of certificates relative thereto.

"The publishing of reports on matters connected with the progress of sanitary science in the United Kingdom and abroad, and the distribution at the end of each year of any surplus funds, or part of any surplus funds, to such institutions as are devoted to the advancement of sanitary science."

If there is one application of science in which all should be interested, it is the endeavour to diminish disease and suffering. Were there no nobler reason, sheer selfishness might well cause all men who claim to be reasoning beings to join, each to the best of his abilities, in helping the cause of sanitary reform. But this is in truth a case where the good of each is the good of all.

That the objects of the Association may be the better effected, it has been formed of two classes—(1) Members who are responsible to a certain amount for the necessary expenses of the Association, and who have a voice in the management of its affairs; (2) Subscribers who incur no liability, and who will take no part in the management of the Association.

The Association not being formed for executing works, leaves the members and subscribers to employ any person they may select to carry out the recommendations of its officers.

Members and subscribers alike contribute an entrance-fee of half a guinea and an annual subscription of half a guinea, or a life-subscription of five guineas, and on payment in accordance with the graduated scale, they are entitled to have one house in London placed on the Assurance Register. The fees for houses outside the metropolitan district will be increased according to distance. The secretary of the Association is Joseph Hadley, Esq., 5, Argyll-place, Regent-street, W.

## PLAIN WORDS IN SCIENCE.†

IN making use of language to express our thoughts, we ought to be sure—(1) That the words used really express the idea which it is wished to convey; (2) that they are the shortest; and (3) that they are the most familiar words which are available. Words must be as objective as possible, *i.e.*, they should bring the subject with the utmost vividness before the mind's eye; and, therefore, those words to which the eye, and the ear, and the mind had been accustomed for the longest time (vernacular terms used from infancy) were the best; and, other things being equal, the shortest words were the best. If the advantages of expressing themselves simply were so obvious, why, it would be asked, do men continue to use the polysyllabic gibberish which passes current as the language of science, but which proves that they have not yet come to a right comprehension of the scientific use of language? By using a language "not understanded of the people" for the expressing of scientific facts, they undoubtedly seriously curtailed the area from which they drew their scientific recruits; and he took it that one explanation of the scientific fervour which pervaded the whole of Germany was to be found in the fact that scientific terms were in that country very largely derived from the German vernacular, and that he who only knows the German language was not necessarily confronted in a German scientific book with words which compelled him to close the volume almost as soon as opened with a sigh of helplessness and hopelessness. It must be admitted that our long words had not hitherto been of much use as a means of international communication. For international communication they must make themselves familiar with each other's languages. That was certain. And it was manifestly of importance that each nation should try to keep its language pure, in order that it might be the more easily learned. The practice of concubinage with the dead languages merely had the effect of producing a mongrel language (as unproductive as are all other mules), of huge bulk and monstrous form, which has to be learnt as an additional study. It seems to be the pitiable ambition of some writers to seize upon a trifling fact, and to give it the longest name they can invent with the aid of a lexicon. Some, possibly, are under the impression that their dictionary-made expressions may gain for them a reputation for classical learning. They cannot afford, as did John Hunter, to rely for their reputation upon the facts which they discover, who, when he was twitted with his want of knowledge of Greek and Latin, wrote thus characteristically to a friend: "Jesse Foote accuses me of not understanding the dead languages; but I could teach him that on the dead body which he never knew in any language dead or living." The defence has lately been put forward for scientific jargon that every trade or profession must have its own technical terms. He confessed he could not see the necessity. The tailor, as far as he knew, derived no advantage from calling his smoothing-iron a "goose"; and seamanship is not advanced because a sailor's "companion" is one thing at sea and another thing on shore. It seemed to him that technical terms ought, as far as possible, to be discouraged, because the coining of new words when they are not wanted, and the giving of strange and conventional meanings to common words, must increase the difficulty of acquiring any art or handicraft. Among unworthy motives which had induced them to have long words, must be reckoned the desire to appear more learned than they were. There was in human nature a tendency which was expressed by the words, *Omne ignotum pro magnifico*—a tendency to put an undue value upon the unknown. It was this natural tendency which led the hero of Warren's famous novel, "Ten Thousand a Year," to make the fatal experiment of applying to his hair the pomade called "Cyanochaitanthropopoion," and it was the same tendency which led the public to buy anything, no matter how common or how worthless, to which the vendor had given a name which was utterly incomprehensible to them. By pandering to this tendency he doubted not that medical terms had been in reality an unspeakable, though delusive, comfort to the public; and that the lady who was told by the physician "that there was still in her husband's lung a perceptible amount of 'whispering pectoriloquy,' although the 'ægophony' had happily completely disappeared," derived from the information the same kind of consolation as did the old woman who, listening to a deep and learned sermon by her rector, found solace in "that blessed word Mesopotamia."—*Times.*

## TRUSTING TO LUCK.

IT is worthy of notice how little those who trust most to chance understand of the laws of chance. This is shown in hundreds of different ways, but by none, perhaps, more than by the strange selections made by the venturesome among the various methods in which they may risk their money: their preference for this or that form of risk, rather than for some other, is scarcely ever based on any real advantage which one form has over the other. Ask a gambler, for instance, to pay £1 for a ticket in a lottery where there are a thousand equal chances and but one prize of £1,000, and he

* "Sanitary Assurance," a Lecture by Prof. de Chaumont. (J. & A. Churchill, London.)

† From an Address by Dr. George Vivian Poore, F.R.C.P., Prof. of Medical Jurisprudence, University College.

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will gladly pay the money. He would most probably not refuse even if there were two thousand equal chances, and the prize were still but £1,000, though the real value of the ticket would be but 10s. If, however, you asked him to pay £1 for the chance of getting £1,000 if a tossed coin comes up head eight times running, he will reject (probably with ridicule) the idea of accepting it. Yet in reality the offer is a far better one than the other. He ought to pay very nearly £2 for the chance offered him for £1 (the exact value of the chance is £1. 19s. 0¼d.). Or the "eight times running" might be changed to "nine times running" if the £1,000 prize were increased to £1,024, and the second offer would then be as fair as the other. But the same gambler who thinks he is quite likely, owing to his luck, to draw the right ticket out of a thousand, would utterly despair of tossing head nine times running.

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"Increased knowledge confers an increased feeling of duty, and increased power to perform it."—*Maudsley.*

The Cedars of Lebanon.—The Vienna *Politische Correspondenz* says:—The once famous cedar forest of Lebanon, formerly so extensive, has dwindled down to the dimensions of a mere thicket, numbering about 400 trees. To save it from complete destruction and preserve it at least in its present extent, Rustem Pasha, the Governor-General of the Lebanon, has issued a special ordinance, containing a series of stringent regulations calculated to check, if not quite put a stop to, the vandalism and carelessness of most travellers. It is expressly forbidden to put up tents or other kinds of shelter within the district of the trees, or to light fires or to cook any provisions in their vicinity. No one is allowed to break off a bough or even a twig from the trees. It is forbidden to bring any beasts of burden, be they horses, mules, asses, or any other kind of animal, within the district. Should oxen, sheep, goat, or other pasturage cattle be found within the prescribed limits, they will be irredeemably confiscated.

Professional Astronomers and Popular Astronomy.—We are inclined to doubt whether the official chiefs of great observatories are, as a rule, the persons best fitted to write treatises on popular astronomy. It is not that they are unwilling to deal with astronomy in a popular manner. On the contrary, they are apt to adopt too familiar and condescending a tone, as if writing for children. But in reality, their astronomical labours, whether in the observatory or in the calculating-room, are not adapted to give them that knowledge of the general science of astronomy, without which no man can present astronomical truths at once simply and effectively. Their work bears the same relation to the real living astronomy of men like the Herschels that land-surveying bears to geology as dealt with by a Hutton, a Playfair, or a Lyell. They find more interest, as a rule, in the correction of a star's place by the tenth of a second of arc than in the inquiry into the star's attributes as a sun. A perturbation affecting the moon's position by the hundredth part of her diameter is more important to them than telescopic evidence of the most tremendous changes in the moon's surface would be. In fine, their way of viewing the heavenly bodies somewhat resembles the way in which a certain Senior Wrangler is said to have viewed Snowdon, who, when asked if he had climbed that mountain, replied that he had not, because a neighbouring hill was equally suitable for trigonometrical purposes. The astronomy thus taught differs as widely from the astronomy of the Herschels as a series of anatomical plates differs from the Venus of Milo or the Theseus and Ilyssus of the Parthenon.

The Induction Balance applied to Surgery.—It was stated in the *Times* of Aug. 5 that the place of the bullet in President Garfield's body had been localised by the use of the induction balance. The *post-mortem* examination seems to have shown that the diagnosis was incorrect. The *Times* Correspondent at the Paris Electrical Exhibition makes the following remarks on the subject, which are interesting as including a concise and sufficiently simple account of the method in question. "I am convinced," he says, "that if the experiments were conscientiously carried out there could be no error. A remarkable confirmation of the utility of the apparatus as a means of diagnosis has just come to my notice, but I will first give a short account of the action of the instrument. A current of electricity is passed through two coils of insulated wire, which are kept apart. The current is rapidly made and broken by clockwork. Above each coil is a second similar coil of insulated wire, and every time that a current is made or broken in the first pair of coils a second current is induced in the secondary coils. These secondary coils are connected together and to a telephone, and the currents induced in either coil tend to produce a noise in the telephone. But these coils are so connected as to induce currents in opposite directions in the telephone, so that under ordinary circumstances they destroy each other, and no noise is heard in the telephone. If, however, a piece of metal be placed inside one of the coils, the character of the induction is changed in that coil, and the balance is destroyed, so that a noise is heard in the telephone. The amount of noise depends upon the form, mass, and conductivity of the metal placed inside. By choosing pieces of metal all of the same size, but differing in their composition, it is possible to compare the conducting powers of the metals. A long scale of metal, wedge-shaped, is moved along above the coil which does not contain the metal to be tested, until the thickness of the wedge over one coil is sufficient to balance the metal inside the other coil. The reading of the scale on the wedge then gives a measure of the conductivity. Some most astonishing results have been thus obtained. It is found that pure copper electrically deposited has a conducting power far greater than the copper of commerce, the difference being far greater than was generally supposed. I will now describe the experiment which has just been completed. Mr. Elisha Gray, of America, whose name is so well known in connection with the telephone and the harmonic telegraph, was a disbeliever in the utility of the induction balance as a surgical appliance. He said to Professor Hughes, 'Thirty years ago, when working at some metal-work, a filing of iron entered my finger; the more I tried to extract it the deeper it went in. I believe it is still there, and if your instrument is of any value, you ought to be able to tell me in which finger it is.' The presence of bone or flesh in the coil of the balance would produce no effect; a metal or other conductor is necessary. Professor Hughes tested Mr. Gray's fingers; none of them gave any sound until he came to the forefinger of the right hand, when the balance of the coils was quite destroyed, and a noise was given out. This was the very finger in which the filing was buried thirty years ago. I need hardly say that Mr. Gray was completely convinced."

Weather Forecasts.—That the daily forecasts issued from the Meteorological Office are often wrong I need not say. Why they are so is simply because general disturbances in the atmosphere only are taken into consideration, local disturbances being ignored. And so long as the forecasts are based solely upon the distribution of barometrical pressure this cannot be otherwise. When there is no distinctly-developed area of high or low pressure lying over the country or approaching our shores, the forecasts must be made at random, for the observer has nothing whatever to guide him in his prediction. He may say "wind light and variable," or "calm," or "weather unsettled"; but such forecasts are liable to falsification in many districts through the operation of local influences. A little hurricane may start up in one spot, rain may pour in another, and a thunderstorm may burst over a third, all unexpectedly, and the observers in the Meteorological Office could not possibly have been expected to warn the afflicted districts. I think, then, the importance of local meteorology cannot be exaggerated. The distribution of rainfall over the British Isles sufficiently shows the potency of physical conditions, but a more striking example is found in the fact that within a radius of twelve miles around Somersham Railway Station (Huntingdonshire) hailstorms are so frequent and destructive that all the insurance companies charge double the ordinary rates per acre on crops growing within that district. Violent winds, too, are often due to local causes. An instance of this came under my notice some years ago in the Isle of Man, when a violent gale blew over Ramsey from the westward, the air a couple of miles to the north being almost still all the time. The storm rushed along the base of North Barrule, lashing the bay into foam, and apparently becoming dissipated out at sea. The afternoon on which this occurred was bright and almost cloudless, and the storm maintained its fury for several hours. Such local disturbances may be quite as destructive as more general ones, and forecasts, to be of any practical use, ought certainly to take cognisance of the causes which produce them. To render forecasts as reliable as our present knowledge can make them, I would suggest that a local observer be appointed in each district to act in concert with the Meteorological Office, and to base the forecasts upon the information and charts supplied by that office, supplemented by his own knowledge of local conditions and influences. Every district has its system of natural weather signs dependent upon its physical features, and experienced farmers, and others who have given attention to them, can predict the weather with far greater certainty than the functionaries in London. There was a time when natural signs were our only aid to a foreknowledge of the weather, but since the invention of telegraphy and the consequent discovery of cyclonic movements in the atmosphere, the cat has been discarded and the barometer put in her place. Why not use both? Theoretical meteorology, so to call it, determines the general distribution of wind and weather over the country; local meteorology, the particular kind of weather in a district. The one is the complement of the other; and in order to obtain the best result they should be used accordingly, the one supplying what the other lacks.—*J. A. Westwood Oliver in the "Times."*

# Our Chess Column.

WE propose to consider the chess openings in the following general order:—

First we shall examine the best methods of attack and defence following from

1. P. to K.4. / P. to K.4. 2. Kt. to K.B.3. / or the *King's Knight's Opening.*

Next we shall examine those following from

1. P. to K.4. / P. to K.4. 2. B. to Q.B.4. / or the *King's Bishop's Opening.*

We shall then take the King's Gambit, examining first the various forms of the opening which follow from

1. P. to K.4 / P. to K.4 2. P. to K.B.4 / P. takes P. 3. Kt. to K.B.3

*the King's Knight's Gambit.* Then those following when White plays 3. B. to Q.B.4 the *King's Bishop's Gambit.*

We shall then consider other openings following from 1. P. to K.4 / P. to K.4 as 2. Kt.Q.B.3 the Vienna opening; 2. P.Q.B.3 the *Q's Bishop's Pawn Opening*; 2. P. to Q.4 the *Centre Gambit*, and so forth.

Next we shall examine the chief openings resulting from

1. P. to Q.4 / P. to Q.4 2. P. to Q.B.4 / the *Queen's Gambit.*

And lastly, we shall examine the various openings called *Irregular*, which include all openings beginning otherwise than 1. P. to K.4 / P. to Q.4 or 1. P. to Q.4 / P. to Q.4 We may note in passing that some of these so-called irregular openings deserve in reality to be regarded as more regular, because steadier and surer, than many of the openings classed as regular.

We do not propose to give a deep analysis of any of these openings, because our wish is to be of use to the many, not to cater for those who are already first-class players. We wish, in fact, to give just so much of what is called book-knowledge of chess as may enable amateur chess-players to start each game on sound chess principles. We wish also to show how, when they meet opponents of superior book-knowledge, they may avoid those openings in which book-knowledge alone can save them from disastrous defeat.

We shall occasionally deviate from this course to discuss the various points illustrating general principles of play, as such points may be suggested by communications received from our correspondents.

## King's Knight's Opening. (Two Knights' Defence.)

There are only three sound replies to the move 2. Kt. to K.B.3; viz., first, 2. Q.Kt. to Q.B.3.; secondly, 2. P. to Q.3; and thirdly, 2. Kt. to K.B.3. of which the third is rather a counter-attack than a defence. There is a fourth way of meeting the move, by 2. P. to K.B.4. (called Greco's counter-gambit), which, though not strictly speaking sound, is often used with great effect by an experienced player against one unfamiliar with the opening. We shall have therefore to consider it, though briefly, further on.

We take first the line of play which we should advise the learner nearly always to follow till he has become tolerably familiar with its various sequels, viz.:—

1. P. to K.4 / P. to K.4 2. Kt. to K.B.3 / Kt. to Q.B.3

The continuation is now either 3. B to Q.B.4. leading to a number of the most interesting openings known in chess or 3. P. to Q.4. the Scotch Gambit, or 3. B. to Q.Kt.5. the strong Ruy Lopez attack, or 3. P. to Q.B.3. a quiet continuation. If the move 3. B. to Q.B.4. is met by the same move on Black's side (Black is for convenience regarded always, in these analyses, as second player), we have an opening which may be resolved into the *Giuoco Piano*, or Steady Game, by 4. P. to Q.B.3. or 4. P. to Q.3, or into the brilliant Evans Gambit by 4. P. to Q.Kt.4.

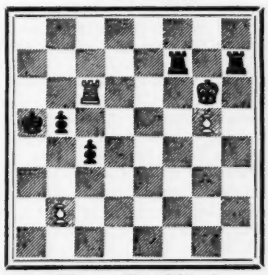
Against a player well acquainted with the openings, the safest reply to 3. B. to Q.B.4 is, perhaps, 3. Kt. to K.B.3, which constitutes the Two Knights' Defence; for this move saves the second player from the Evans Gambit, to meet which requires thorough knowledge of a great number of varied lines of play, while declining the proffered Queen's Knight's pawn subjects the second player to a cramped defensive game. We do not curselves advise 3. Kt. to K.B. 3 for general adoption, for the games to which it leads are seldom so interesting as those following from 4. B. to Q.B.4. But, properly played, it is a safe defence. And as the variations following from it are not nearly so numerous as those from 4. B. to Q.B.4, it will be well to dispose of it before entering on the latter.

We shall begin the inquiry into the Two Knights' Defence next week.

In the meantime we give for consideration two positions.

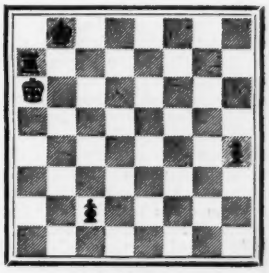
No. 1, which occurred a few nights ago in a game in which we had given the odds of a Queen to a lady.

No. 1. Actual End-Game.
Black. (Lady).



White. (Chess Editor.)
White to play and draw.

No. 2. By Mr. F. Healey.
Black.



White.
White to play and draw.

Black's last move was R. to K.R. 2, imprisoning the King, while leaving the two Black Rooks free to move *ad lib.* along Black's second row. This move, however, enabled White to draw the game. The young player will readily see how.

This, of course, as occurring in ordinary play is not to be regarded as a Chess study. It simply illustrates one of the dangers which a weak player, who, after a game at odds, finds himself with a winning superiority of force, should carefully avoid. Position No. 2, by the celebrated problem-maker, Healey (which appeared in the *Westminster Papers*, as nearly as we can recollect, in 1874) illustrates the same danger very prettily.

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[1.] Chess Query.—In an old number of the *Illustrated London News* I find the following problem given by the Chess Editor (then Mr. Staunton, I believe), as a puzzle, by the Anonymous Modenese. White K. at Q.Kt.sq.; Q. at K.B. 4; R. at K.Kt.5; B. at Q.Kt.2; Ps. at K.B.5, Q.Kt.3, and Q.R.2. Black K. at K.R.sq.; Q. at Q.4; R. at Q.sq.; Kt. at Q.R.5; Ps. at K.R.2 and K.Kt.2. White to play and Mate in four moves. Can any one give me the correct position? There seems to me to be no solution with the position as given.—Queen's Knight.

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XUM

# Our Whist Column.

By "Five of Clubs."

### Introduction.

WHIST, properly played, is the finest of all card games, perhaps — not even excepting chess — the finest of all sedentary games. But Whist, as it is often played, without any knowledge or appreciation of the real nature of the game, seems to have nothing making it better worth playing than Pope Joan or Casino, and to be decidedly inferior to Euchre. There is an intermediate kind of whist, the game played by persons who have a keen perception of the strategy of the game, but no knowledge of its language, which may be full of interest or full of annoyance, as the cards may happen to lie. To watch a proficient in this kind of whist, playing a good hand, and ably supported by a steady-going partner who understands his ways, one would say Whist was the most delightful of all games; but to see him playing an average hand, and to note his wrath when his partner, considering his own hand, fails to play precisely as he wishes, one would say Whist was a rather severe form of punishment.

The present series of papers on Whist, and the problems, games, &c., which will accompany and follow it, are intended to indicate the nature of the only game of Whist which is worth playing—Whist as a game between two forces, two pairs of partners, each pair having between them twenty-six cards. The game thus played, that is with constant reference by each player to the fact that he has a partner, may be regarded as a really scientific game. It is often called the book game, theoretical play, and by other names, implying that a fine player need care very little about it. But it is, in truth, the only common-sense, practically-sound form of the game, and no one can be regarded as a really good, still less as a fine, player who does not play it. It has, moreover, the additional advantage of being readily learned by those who have not the capacity for really great play; and when it has been learned, such players, though never brilliant, become good and safe partners. Moreover, by learning the rules of scientific Whist, which seem at first an extra trouble to the memory, the learner finds that his power of remembering the fall of the cards is greatly increased. It is, indeed, the purposeless nature of ordinary unscientific Whist play which makes it so difficult for the bad player to remember what cards have been played, and by whom. So soon as he has adopted just principles of play, each hand is played according to a plan, the development of which is full of interest, so that the stages are easily remembered. Each card is played with a purpose, and whether the purpose succeeds or fails, the result is noted and remembered, whereas when there is no purpose, the memory has no such aid.

The first great principle of the scientific game of Whist is to give your partner (always at the beginning, and almost always throughout the play of the hand), all the information in your power within the rules of the game. (There arise cases occasionally towards the end of a hand where it becomes clear that the partner can do nothing, and nothing can be lost by misleading him; then, and then only, false cards, deceiving him, but deceiving the adversaries also, may be usefully played.) To this the objection is repeatedly made—especially by brilliant one-handed players—"a player has but one partner while he has two adversaries, and by playing so as to give information to one friend, he gives information to two enemies, or the harm exceeds the good two-fold." The true answer to this objection does not seem to me to have been recognised by Pole, Cavendish, Clay, and other great masters of the game, who have yet, of course, known perfectly well from practice that it is advantageous to give to your partner all the information in your power. Cavendish says the objection would have considerable force if you were compelled to expose the whole of your hand, but you possess the power of selecting what facts shall be announced and what concealed. Pole says the objection "involves a confusion in reasoning; for if the opponents are equally good players, they will adopt the same system, and the positions must be equal; and if they are not good players, they will be incapable of profiting by the indications you give, and the whole advantage will rest with you; adding that "even good players seldom pay so much heed to their opponents' as to their partners' indications." Pole and Drayson agree in saying that by not giving your partner information, you run the risk of having to fight three opponents single-handed. Clay does not specifically consider the objection.

The true answer seems to me to be different from any of these. The reply of "Cavendish" implies that there is a limit to the principle that it is more important to inform your partner than to deceive your adversaries. Pole's reply takes the system for granted, by assuming that good opponents will follow it; and certainly he does not reason soundly in suggesting that even good opponents pay less attention to their opponents' than to their partner's indications. It is also an exaggeration to speak of a partner as becoming a third opponent if not duly informed as to your cards: he may take one or two of your winning cards, but cannot play as an opponent throughout the hand, as they seem to imply. (The mischief is bad enough, without exaggeration.) The real reason why information to your partner is so important as to outweigh the knowledge given to the adversary, is that it is only by giving him information that your cards can be combined with his in the strategy of the hand. You tell him points about your hand which he can utilise, let the opponents do what they will, although, of course, you may also give him information which he cannot utilise, whether because the adversaries have also learned it, or not. Cases of the latter kind count neither one way nor the other; if you had not suggested such and such a plan, he would not have tried it, and when you have told him he has not succeeded, so that you are none the worse; all the cases of the former kind are so much clear gain.

Take a familiar instance. I lead ace, and follow with queen of my best suit. My partner *knows* that I have the knave and a small card left. Suppose he has the king in his own hand and a small one left after the first round. Now according to the state of the score and of his own hand, it may be better to let the trick fall to my queen, or to take it with his king, leaving me still the command of the suit with my knave. By my play, showing that I have the knave, I have left it open to him to do whichsoever of these two things may be best for both of us, and this choice he has, let the opponents act as they please. But suppose that, instead of following the recognised line of play for such cards, I lead the second round with my small card. My partner plays his king, and, let us suppose, wins the trick. He cannot now play as he would (as it might be absolutely essential to success that he should) if he knew that I had the command of the suit. On the contrary, so far as he can understand me at all, he thinks I have three small cards of the suit left, and that the queen lies with one of the adversaries. His consequent play in this case spoils our common game, whereas in the other case his play advances our common game. In either case it is *his* play, not the opponents', which affects our combined game for good or for ill.

In fine, instead of the maxim, "It is more useful to inform your partner than to deceive your adversary," I would substitute this—"Your single partner can do more good than both your adversaries can do harm by utilising information you may give by your play." (Good here includes the avoidance of harm; we might supplement the rule by saying that your partner is likely to do much more mischief through ignorance of your hand, than could be counterpoised by any good which the adversaries might chance to do you.)

It is the recognition by good players of this first rule, as resulting from the general principle that partners should play in harmony and with a common purpose, which has led to the system of modern whist strategy. There are commonly more ways than one in which, if the partner's cards were seen, the qualities of the combined hands might be used; but there is only one system by which, in the actual method of play, your partner can work in harmony with you. That system being adopted, the principles guiding us in the opening of a hand, and determining the play of first, second, third, and fourth player, are deduced at once. Our books of whist seem, indeed (and it has always seemed to me a fault in them), to require that the learner should know multitudinous rules for leading, and for playing second, third, and fourth; but in reality all these rules depend on one general principle. I do not say that the player ought at once to know, from his knowledge of this principle, his proper course as leader, second, third, or fourth player. He has not time to go through all the considerations involved in applying it to particular cases. He must be content, therefore, to retain a number of rules for such cases in his memory. But his memory will be greatly helped, and the number of rules will be greatly diminished, when he recognises the general principle on which modern whist-play proceeds.

In my next I shall show what this general principle is, why it has been selected in preference to others, which, at first view, seem to have great, if not equal advantages. Afterwards we shall consider how this principle suggests the various leads, the play of second, third, and fourth hand, &c., endeavouring so to treat the matter that the memory may be as much as possible helped to retain the resulting rules, by recognising the string on which these seemingly scattered beads of Whist wisdom are in reality strung.

I may in the next number present a game, actually played, mentioning the inferences which a player of the systematic game would make at once. Those who follow our explanation of the system will presently see that these inferences are not, as they might suppose, recondite, but perfectly obvious, even after a very moderate study of the modern system of Whist play.

XUM